

ESSAYS

ON THE

SOURCES AND MODE OF ACTION

OF

FEVER.

---

BY WILLIAM DAVIDSON, M. D.,

SENIOR PHYSICIAN TO THE GLASGOW ROYAL INFIRMARY, ETC. ETC.;

AND

ALFRED HUDSON, M. B., T. C. D.,

PHYSICIAN TO THE NAVAN FEVER HOSPITAL.

---

PHILADELPHIA:

PUBLISHED BY A. WALDIE, 46 CARPENTER STREET.

1841.

ESSAYS

ON

SOURCES AND MODE OF ACTION

FEVER

BY WILLIAM DAVIDSON, M.D.

ALFRED HUDSON, M.A., F.R.S.

REPRINTED FROM THE LANCET

CHICAGO, ILL.

PUBLISHED BY A. W. H. & CO., 111 N. WABASH STREET

1881

# I.

## THACKERAY PRIZE ESSAY.

### *Essay on the Sources and Mode of Propagation of the Continued Fevers of Great Britain and Ireland.*

BY WILLIAM DAVIDSON, M. D.,

Senior Physician to the Glasgow Royal Infirmary, Lecturer on Materia Medica, Member of the Faculty of Physicians and Surgeons of Glasgow, &c.

---

## CHAPTER I.

### On the Sources of Continued Fevers.

Numerous kinds of continued fevers have been described by authors; but many of these have been found, on investigation, only particular varieties, in place of being distinct species. This has been particularly the case with typhus, the most prevalent kind of continued fever in this country; and it may be accounted for, from its numerous and diversified complications giving rise to various and multiform symptoms. The pathology of typhus, however, of late years has been considerably advanced; and it is now established, that this disease may be either simple or complicated, with organic affections of one or all of the different cavities of the body.

We shall not, at present, enter into any discussion respecting the various kinds of continued fever that are to be met with in the United Kingdom; but as perspicuity in arrangement requires some classification, we shall adopt the following, reserving the illustrations upon which this classification is founded for a future part of the essay:—

1. Typhus.
2. Febricula or Simple Fever.
3. Gastric or Intestinal Fever.

These affections seem to be distinct species of disease, differing in their symptoms, causes, and laws; and are generally treated in private practice and in hospitals as continued fevers. To this list



may, perhaps, be added bronchitis, which, although an inflammatory affection, is more frequently confounded with them than any other disease.

### SECT. I.—*Sources of Typhus.*

It appears to us quite unnecessary here to describe what is understood by typhus fever; as we assent to the general correctness of our standard authors upon this subject, and as some of their descriptions will be quoted in another part of this essay. At the same time, it may be remarked that typhus possesses an advantage over the other forms of continued fever, in having a distinctive characteristic, viz. the eruption, which is present in none of the others, and which is now almost universally acknowledged as decisive of its existence. It must, however, be admitted that typhus can and does occur, without its characteristic eruption; but it is equally certain that the large majority of patients who have decidedly the general typhoid symptoms, are more or less spotted with this efflorescence. It is therefore the sources of typhus, as generally so characterised, which we mean to trace in this section.

There is considerable diversity of opinion amongst British physicians respecting the causes of continued fevers; but certainly the majority of authors have adopted the belief that typhus is propagated by contagion. The opinion of the majority appears to be supported by the facts connected with the progression of the disease; it shall therefore be our object to establish this point. It is not intended, however, to enter into any speculations respecting the primordial source of the contagion of typhus; for the sources from which it, as well as that of other contagious fevers, originated, is involved in absolute obscurity; and though we could trace them to the most remote era in antiquity, the same difficulty would be encountered. Some authors, apparently to get rid of this difficulty, and to account for the occurrences of typhus where no contagion could be traced, have adopted the opinion that it may be generated by common causes, such as impure air, filth, &c., and be afterwards capable of propagation by contagion.<sup>1</sup> The argument of analogy is directly opposed to this belief; for if, in nature, there be no exception to the law, that two causes are never required to produce precisely the same effect, it will follow that whatever cause can be best reconciled with the phenomena of typhus, must be considered the true source of the disease. But, in order to apply this principle more immediately to the subject, it may be necessary to appeal to the various morbid poisons, the laws of which are known and generally admitted. The first we shall notice, are those which are admitted by all writers to be propagated by one cause only;

<sup>1</sup> The terms contagion and infection are used synonymously, as indicating the ponderable or imponderable matter, which is generated in a diseased living body, and which is capable of producing the same disease when applied to another.



viz. matter, whether ponderable, as the pus contained in a variolous pustule, or imponderable, as the effluvia issuing from a patient labouring under small-pox. Measles, scarlet fever, whooping-cough, are propagated only by the *effluvia* which are generated by the patient; and though the material body by which it is effected cannot be collected into vessels, like the various gases, still the proofs, upon which their contagious qualities are based, are as unquestionable as those of small-pox. Almost all the contagious diseases of the skin, such as syphilis, scabies, the yaws, siverens, &c., furnish examples of propagation by only one cause, viz. contagion. There are no doubt authors who maintain that some cutaneous diseases are generated by filth, &c., such as some of the infectious species of porrigo; but of this there is no proof; and in all probability it is equally unfounded, as was a similar hypothesis respecting the origin of scabies. These diseases always retain the same characteristics; the one is not convertible into the other; and no known combination of them can generate a new contagion capable of perpetuating a new disease. A specious objection might be brought forward against the introduction of an analogy from chronic contagious diseases, which are only propagated by contact, according to the general belief; and besides are regulated in other respects by different laws, whereas typhus is propagated only by effluvia. It is quite evident that a class of diseases may be recognised by one leading and undeviating law, while they differ in many of their subordinate characters; and yet this peculiar law of similarity between them may be as certain and definite as if they had been united into one family by all their habits. The effluvium which issues from a small-pox patient must be as essentially matter, as is the fluid of a variolous pustule; for though the first cannot be collected in a separate form, it must possess one or other of the properties of small-pox matter, else it could not induce the disease; the only difference between them consisting in this, that the contagious matter is fluid in the one case and effluvial in the other. It is necessary that the foregoing observations respecting contagious diseases be kept in view; for upon the analogy between them and typhus we mean to establish an argument, that the latter disease can only be propagated by one cause. If it be true, then, that all the contagious fevers known can be propagated only by contagious matter, and by no other cause, however much their contagious qualities, their prevalence, and their fatality may be increased by other causes, it must follow from the law of analogy that if typhus can be proved to be contagious, it must also be propagated only by one cause, viz. contagion. We shall, therefore, endeavour to prove this point; and, in the outset, it may be stated, that we do not mean to fatigue the reader by stories about fomites and persons who have carried the contagion about them for months or years, nor to hunt out a particular individual who has conveyed it from one town to another. In place of accumulating evidence of this kind, which although sometimes very conclusive, is in other cases somewhat

questionable, we shall select a few facts from the history of our British and Irish hospitals, which, we trust, will be sufficient for our purpose; for if it can be established from these documents that the disease was contagious in all the large hospitals of Britain and Ireland, then it must be more or less so in every other place. In selecting the facts, we shall adduce the most conclusive instances, such as where the whole, or almost the whole, of the attendants of the patients affected with typhus were infected; for were the whole body of evidence existing on this subject accumulated in this essay, the argument would be encumbered, and the proofs perhaps rendered less convincing. Those who deny the existence of typhous contagion, may assert that this is unfair, and that those hospitals also should be brought forward, where the medical and other attendants were rarely affected. As we shall, however, notice this and other objections elsewhere, it need not be farther alluded to here. Drs. Barker and Cheyne, in their admirable report of the fever which prevailed in Ireland during the years 1817-18-19, state that "in the hospitals of the House of Industry of Dublin, no clinical clerk or apothecary escaped an attack of the disease; and on the 20th of January, 1819, it was reported to government that five of the medical attendants of the House of Industry were at that time lying ill of the disease. In the city of Cork, nine physicians, in attendance either on dispensaries or fever hospitals, were attacked; every medical attendant at the South Fever Asylum, in that city, suffered. At the hospitals of the House of Industry, one hundred and seventy persons were employed in different offices of attendance on fever patients; and from this part of the establishment were recorded one hundred and ninety eight cases of fever." In Dr. Crampton's medical report of the department of Steevens' Hospital, it is observed, "that, with the exception of Dr. Harvey and himself, all those concerned in attendance on the patients caught the disease; none of the nurses, none of the porters, barbers, or those occupied in handling, washing, or tending on the sick, escaped, and many of them had relapses and recurrences of fever."

Dr. Bracken, in his report of the Fever Hospital of Waterford for 1818, states that "there were twenty-seven attacks and relapses of fever among the nurses, servants, and porters, whose number fluctuated according to the demand for them, but who, on an average, may have been about twenty-two during the year." He farther states that "the present year, 1819, bears a close resemblance to the last, in respect to the nurses and servants being attacked with fever, eighteen of the former having suffered under the disease; seven of them once, three twice, and one three times. The apothecary, who had not been long in the hospital, caught fever and relapsed twice. During his illness, a young man, who performed part of his duties, was attacked after a short attendance. A temporary apothecary was then engaged for a few weeks; but he

<sup>1</sup> Barker and Cheyne's Report of the Fever in Ireland, vol. i. p. 135.



had not been many days in his new employment when he also contracted a fever."<sup>1</sup>

Drs. Barker and Cheyne remark that clergymen, who visited typhus patients in Ireland during the epidemic, were also observed to suffer in a very remarkable degree; and they quote the following passage from Dr. Stokes's *Essay on Contagion*, which was published at a time when the fever had made little progress in Dublin:—"The deaths from fever recorded in Saunders's News-Letter, from August 1st to December 12th following, are sixty-four, and of these nineteen are of clergymen of some of the different persuasions, or of medical men of different descriptions, which appear greater than the proportion which these two classes bear to the whole of those whose deaths we may suppose were mentioned in that manner."<sup>2</sup> Dr. Tweedie, in his *Clinical Illustrations of Fever for 1828-29*, observes that "the London Fever Hospital is placed in an open space, situated in the vicinity of the metropolis, close to the Small-pox Hospital. Both these establishments stand in the centre of a large field, where the production of malaria is extremely improbable. I can state from the most authentic sources, that every physician who has been connected with it, with one exception (the late Dr. Bateman), has been attacked with fever during his attendance, and that three out of eight physicians have died. The resident medical officers, matrons, porters, laundresses, and domestic servants, not connected with the wards, and every female who has ever performed the duties of a nurse, have one and all invariably been the subjects of fever; and to show that the disease may be engendered by fomites in clothing, the laundresses, whose duty it is to wash the patients' clothes, are so invariably and frequently attacked with fever, that few women will undertake this loathsome and frequently disgusting duty. Last summer, a most convincing illustration of contagion occurred. The present resident medical officer was attacked with fever, and it was necessary, in consequence, to appoint some one to perform his duties during his illness. The first person who officiated for him resided constantly in the house during the day, but took the precaution of sleeping at home. He was, of course, very much exposed in the wards in the performance of his duties. These, however, were soon interrupted by an attack of fever, which confined him for a considerable time. The duties were then undertaken by a medical pupil, who had completed his education, and entered the hospital in the most robust health. He had been taught, and did implicitly believe, in the non-contagious nature of fever, and ridiculed the idea of any personal danger from residing in the hospital. He performed the duty of house-surgeon for ten days only, when symptoms of a severe fever appeared."<sup>3</sup>

<sup>1</sup> Barker and Cheyne's *Report of the Fever in Ireland*. vol. i. p. 276.

<sup>2</sup> *Ibid.* vol. i. p. 138.

<sup>3</sup> Tweedie's *Clinical Illustrations of Fever*, p. 87.



Dr. Tweedie also adduces some important facts connected with the fever which prevailed in Edinburgh during the year 1817, which are the following:—Owing to the prevalence of fever at Edinburgh in 1817, it was necessary to apply to government to permit Queensbury House to be employed as a fever hospital: “In the immediate neighbourhood of this extensive building fever was decidedly less prevalent than in any other quarter of the town. All those, however, who resided in the hospital, including the resident house-surgeon, clerks, apothecary, and nurses, were successively attacked.” The following is Professor Alison’s report on this subject. “When Queensbury House was formerly occupied by fever patients, every resident clerk and every nurse in the house were successively affected with the disease; and since it was reopened in December last (1826), the resident physician, two of the clerks (who have not been resident, but have been several hours in the day in the house), the apothecary, several servants, and all the nurses except two, in all above forty individuals, who had necessarily close intercourse with the sick there, have had fever. If this be the effect of a malaria, it must be a very virulent and effective one, and it is reasonable to expect that some record of similar visitations in the former history of the building would be found. But Queensbury House has existed for about a century; it was long occupied as a private dwelling-house by the noble family of that name; afterwards it was occupied by a number of families, and afterwards as a soldiers’ barrack; and yet no record can be found of its having been, during these changes, the seat of an epidemic fever. If a malaria has existed, therefore, in that house, it must, on both occasions, have sprung up exclusively at the times when fever patients were removed thither and lasted only during their stay. During the present epidemic (1827–28, as well as that of 1817–19), many of the clerks and nurses employed in the Royal Infirmary have taken fever. Since November last, six of the clerks employed in the clinical wards only, four of those employed in the ordinary wards, and twenty-five nurses or servants have taken fever. All these persons had necessarily frequent and close intercourse with the fever patients in the house, having been employed more or less constantly in the fever wards, excepting only four of the servants. Of these four, two had been employed in the laundry, where the linen from the fever wards was washed. One was a porter employed at the gate, who would, of course, have communication with the fever patients at their entrance and dismissal, as well as with their relations coming to visit them; and one was a nurse employed in the servants’ ward, but who was in the habit of visiting the fever wards.” He adds further: “No one of the nurses, whose duty has confined them to the medical or surgical wards, where no fever patients were admitted, has taken fever, with the single exception of the woman in the servants’ ward above mentioned; and of the numerous patients in these ordinary wards, the only one who has taken fever, within my knowledge, during the

present year, was a patient in the men's general clinical ward, who lay in the bed next the door that communicates with the clinical ward."<sup>1</sup> Dr. West, in his account of the cases of typhus exanthematicus that occurred in St. Bartholomew's Hospital in 1837-38, states that "since last summer, eleven gentlemen who were in the habit of frequenting the hospital have been attacked by the fever, to which three have fallen victims; sixteen nurses and twenty-one patients admitted for other affections, have likewise suffered from the disease, which terminated fatally in ten instances, and I do not doubt but that many similar cases occurred which did not come under my notice."<sup>2</sup> Dr. Roupell, also, gives similar testimony in reference to St. Bartholomew's Hospital, and states that "amongst the nurses in attendance upon the sick, in that establishment, infection was almost universal."<sup>3</sup> In the Glasgow Fever Hospital, which is capable of containing 220 patients, during the last six or seven years almost every clerk and nurse of that establishment have caught fever while acting in the wards, unless they had previously laboured under the disease. On the other hand, the nurses connected with the medical and surgical wards, in the adjoining building, have almost uniformly escaped. Occasionally a case has appeared in the medical and surgical wards; but this fact ought to be coupled with the statement, that now and then typhus cases are sent, by mistake, into the medical wards, and cases of bed-sores, gangrene of the feet, &c. are transmitted from the fever hospital to the surgical wards. Dr. Cowan states that "All the gentlemen who have acted as clerks in the fever hospital for many years past have been attacked with fever, unless they had it previously to their election. During last year, twenty-seven of the nurses of the establishment were seized with fever, and five of them died: several of the students have been affected. One gentleman who acted as apothecary died in the house; and if I have escaped, it must be attributed either to being past the period of life at which fever usually takes place, or to my being secured by having had two dangerous attacks at an earlier period of my career, when acting as physician's clerk in the infirmary, during the epidemic of 1816-17-18."<sup>4</sup>

Dr. Mateer gives a table of 9,588 cases, which were admitted into the Belfast Fever Hospital, from 1818 to 1835, showing the number of patients who had any communication with affected persons, either by residence in the same house, or by belonging to the same family. He draws the following conclusion from the table: "It thus appears that the number of families where contagion is traceable is 1,856, that the total number of persons belonging to them is 7,246, making an average of nearly four individuals to each family;

<sup>1</sup> Tweedie's Clinical Illustrations of Fever. Edinb. Med. and Surg. Journal, vol. xxviii. p. 238.

<sup>2</sup> Ibid., July, 1838, p. 143.

<sup>3</sup> Roupell on Typhus.

<sup>4</sup> Cowan's Vital Statistics, p. 26.



and that the single cases, where the disease seemed to have arisen from other sources, amount only to 2342.<sup>21</sup>

This assemblage of facts has been drawn from the large hospitals in England Scotland, and Ireland, and the observations have been made during various years and during different epidemics by gentlemen of the highest talents and respectability; their authenticity cannot, therefore, for a moment be questioned. The simple relation of these facts would, we think, with the majority of men, produce conviction that fever was at least contagious in these hospitals, provided the mind was not preoccupied with an opposite theory; but a few observations will tend to produce a proper estimation of this testimony. It is quite obvious, that where a much larger proportion of persons is affected with any particular disease, in any particular place, than occurs amongst the general community, or in any particular grade of society, there must be some local cause for that increased ratio. This has, manifestly, been the result in the fever hospitals that have been enumerated; for in all, a very large majority of the attendants, and in some the whole of them, were affected with fever. Now, no one will contend, that even amongst the lower classes (who generally suffer from fever to the greatest amount), such a proportion has ever been maintained, even in our most severe typhoid epidemics; but if the number of hospital clerks be taken and compared with the unaffected number, in the particular grade of society to which they belong, such an attempt would be ridiculous; for the united testimony of the hospital physicians of England, Scotland, and Ireland (whose statements we have already quoted) amounts to this, that almost every clerk of a fever hospital has laboured under fever during some period of his attendance upon it. It may be contended, in answer to this argument, that the atmosphere of the hospitals was contaminated by the exhalations arising from the number of patients and the want of proper ventilation; but the same process of atmospheric deterioration ought to take place, in the medical and surgical wards, if they be equally filled, which is generally the case; and in the latter wards there are often, in addition to the ordinary exhalations, the effluvia arising from wounds, ulcers, &c., yet typhus rarely, and only in sporadic cases, springs up there.

The opponents of contagion, however, endeavour to explain the prevalence of fever among hospital attendants by the hypothesis, that the same cause that produced it in the filthy, ill-ventilated houses of the lower classes is in existence in these institutions; viz., a peculiar malaria, generated chiefly in large towns. If this hypothesis were true, it would follow, as a necessary consequence, that the other parts of the building, being similarly situated, would be subjected to the same malarious effluvia, and hence its inmates would be affected with the same kind of disease; but this has never occurred in any of the large hospitals already alluded to, nor in any other,

<sup>21</sup> Dublin Journal of Medical Science, vol. x. p. 35.



so far as we are aware, where patients affected with typhus are kept exclusively in one place. Again, it may be asserted by the non-contagionist, when driven to the last extremity, that though malarious effluvium be not generated in an hospital, it may be carried there by the clothes of the patients, and the attendants may be infected by coming into contact with them. The analogy of malarious diseases is in opposition to this belief; for it is not found that a patient labouring under ague infects any person who has not been in the malarious district; neither, according to the general belief, does a patient labouring under yellow fever, when removed from the quarter where he caught the disease, excite contagion in the vicinity of his new residence. But as this supposed typhoid malaria may be assumed to possess something *sui generis*, an argument stronger than analogy can be adduced, viz., the impossibility of carrying any principle of that kind into the wards. It is the practice, in many of the large fever hospitals, to remove the clothes of the patients, to bathe them, shave their heads, and give them clean linen, before they are sent into the wards. This plan is adopted in the Glasgow Fever Hospital, and the following is one of the regulations of the Waterford Hospital.

Dr. Bracken states, that "according to one of the regulations of the hospital, every patient has his hair closely cut at the time of his admission; he is also well washed with warm water and soap, and supplied with linen before he enters the sick ward."<sup>1</sup>

## SECT. II.—*On the Analogy of Typhus to Exanthematous Fevers.*

Having discussed the most important and specious hypotheses which have been brought forward to explain the general and ultra-proportionate prevalence of fever among the attendants of the sick in large hospitals, independent of the operation of contagion, we shall take notice of the general objections to the doctrine of contagion in typhus fever; and these may be comprehended in the statement, that it is not characterised by the laws of other contagious fevers. Before entering upon this part of the subject it may be remarked, that more importance is generally attached to this argument than it merits; for though, in the absence of facts, analogy is the most conclusive process of reasoning that can be employed, yet, undoubtedly, when facts are opposed to the application of this principle in any individual case, the facts have the preponderance over the analogy. And, though we were unable to prove that typhus was analogous, in its leading characteristics, to the contagious exanthemata, yet if it be admitted that there is no theory which can explain satisfactorily the facts regarding the prevalence of the disease in hospital attendants, except that of contagion, the case would be conclusively determined, even in opposition to the analogy, and would be set down either as an exception, or as one

<sup>1</sup> Barker and Cheyne on Fever, vol. i. p. 259.

of a new series of contagious diseases. It fortunately happens, however, that the law of analogy will be little violated by comparing typhus with the contagious fevers, for in its leading characteristics it resembles them pretty closely; at the same time it ought to be observed, that typhus fever has only, of late years, been examined with sufficient care, as to many points, connected with its history, laws, and pathology, and that it labours under the disadvantage of being frequently confounded with other continued fevers, to which, in its early features, it bears an intimate resemblance; so that the same certainty of analogical conclusion cannot be expected, as exists among the other exanthematous fevers. We shall endeavour, however, to show, by the facts which shall be quoted, that typhus comes distinctly within the range of their analogy, and that though it is not so regular in its progress, nor so certain in its eruption as smallpox or measles, yet that it differs as little from scarlet fever, in these respects, as the latter differs from smallpox.

The principal laws of the contagious exanthemata are the following:

1. The contagion can be traced in families, hospitals, schools, &c., and those exposed to it are very generally infected.

2. They only affect persons once during their lives.

3. They are characterised by an eruption, which has a rise, progress, and decline, and the disease cannot be checked in limine.

1. *The contagion of typhus is traceable in hospitals, schools, families, &c.* In determining the contagious nature of any disease, it is not necessary that we should be able to trace every case, or even the majority of a particular amount of cases, to a communication with an infected person, or to exposure to a particular fomites; for this would imply that we could, like an American Indian, discover the trail of a patient and trace him through all the windings of a large city, and, besides, should investigate the history of every individual whom he has passed or rubbed shoulders with in every narrow and dirty alley. Even in smallpox, measles, and scarlet fever, any attempt of this kind to trace the contagion regularly would be fruitless, and for the very same reason. Smallpox was, at one period, believed by some authors to originate in filth, because it was found impossible, in numerous cases, to account for its existence in certain localities upon the principles of contagion. Dr. Adams remarks, that "many children born in London live for several years without receiving the smallpox. In the same neighbourhood a person arrives from the country, and without any apparent intercourse with an infected person is attacked by the disease."<sup>1</sup>

Independent of the many exposures to infection, which are perfectly unknown and undiscoverable by the patient, it is very difficult to ascertain the facts connected with the ordinary movements of a patient, which, in many cases, can only be elicited by tedious

<sup>1</sup> Adams on Morbid Poisons.

cross-examinations ; so that this method of determining the point is liable to many objections, and greatly inferior in conclusiveness to the evidence derived from the spread of a disease in any large school or hospital ; but, certainly, it tends to prove the doctrine of contagion in typhus as much, if not more, than it does in smallpox, &c., as will appear from the following table. The whole of the eruptive cases of typhus, in which this point was investigated, and that were admitted into the Glasgow Fever Hospital from 1st May to 1st November, 1839, are included only in this table, the males and females being classed together.

|                     | Exposed to Contagion. | Uncertain. | Cold. | Total No. of Cases. |
|---------------------|-----------------------|------------|-------|---------------------|
| Eruptive typhus . . | 201                   | 169        | 53    | 423                 |
| Febricula . . . .   | 10                    | 28         | 22    | 60                  |
| Smallpox . . . .    | 7                     | 19         | 1     | 27                  |
| Scarlet fever . . . | ...                   | 4          | ...   | 4                   |
| Measles . . . . .   | ...                   | 2          | 1     | 3                   |

The number of eruptive cases of typhus admitted into the fever hospital, both in the period included in the above table and also during the previous six months, who have been exposed to contagion, we have always found greater than in those affections not characterised by the exanthema ; and it is remarkable that, notwithstanding the most careful inquiries, only seven cases of smallpox could be traced to contagion out of twenty-seven.

Dr. Cowan states, that "of the patients admitted into the fever hospital last year, 472 males and 589 females, forming 47 per cent. of the whole, either ascribed the origin of their disease to contagion, or had been exposed to its influence."<sup>1</sup>

The propagation of the typhoid contagion is also intimately connected with filth and deficient ventilation ; and there are few medical facts better ascertained than the close connection of pestilence with these circumstances. Dr. Hancock remarks, that "the connection of plague with filth and impure air, and crowded ill-constructed cities, and with certain seasons and climates and states of the atmosphere, calculated to engender mischief, though not accurately defined, has been so repeatedly observed in different countries as to stand on a far more solid foundation."<sup>2</sup> Dr. Bateman, in his *Historical Survey of the Diseases of London*, states that "Dr. Heberden has collected the most ample and satisfactory evidence of the connection of the plague, and of the malignant contagious fever which generally precedes and accompanies it (if, indeed, they be not modifications of the same disease) with the filth of crowded, ill-ventilated large cities, in all ages and countries." He then quotes Dr. Heberden's remarks : "It has always originated and maintained its head-quarters in the filthiest parts of those cities ; as in St. Giles's,

<sup>1</sup> Cowan's *Vital Statistics of Glasgow*, p. 26.

<sup>2</sup> Hancock on *Pestilence*, p. 224.



in London, in 1665, and in Whitechapel in 1626 and 1636; and in those cities of Europe, which, from natural or political causes, have been backward in adopting the improvements of modern times: the picture of former manners is not exhibited in more lively colours than that of former diseases. The plague visited Denmark in 1764, it raged at Moscow in 1771, and at Cracow still later. The last-mentioned town, Mr. Wraxall says, was not wholly paved till within the last two years, and nothing can be so execrable as the present paving, which scarcely deserves the name. There is not a single lamp in the place; no precautions are used to cleanse the streets, which, of course, become infectious in summer and almost impassable in winter." The following is Erasmus's description of the habits of the English, about two centuries ago: "The floors are commonly of clay, strewed with rushes, which are occasionally renewed, but underneath lies unmolested an ancient collection of beer, grease, fragments of fish, spittle, the excrement of dogs and cats, and every thing that is nasty."<sup>1</sup> Dr. Hancock observes, that most writers on the plague have remarked the exemption of Persia from this disease, and he quotes the following passage from the City Remembrancer: "The Persians, though their country is every year surrounded by the plague, seldom suffer any thing by it themselves; they are the most cleanly people in the world, many of them making it a great part of their religion to remove filthiness and nuisances of every kind from all places about their cities and dwellings."<sup>2</sup> Drs. Barker and Cheyne, in their statement of the circumstances which either preceded or attended the epidemic fever in Ireland during the years 1816 and 1817, make the following remarks, which may be assumed as conclusions drawn from the reports of physicians practising in the various provinces, and from the observation of more than 100,000 cases in general hospitals: "When fever commenced in a poor family, or was introduced by a stranger or lodger, it generally extended to all its members. The poor were the chief sufferers, in consequence of their neglect of cleanliness, particularly with respect to their clothing, and the smallness and crowded state of their apartments; evils, at the time, much increased by the extreme poverty which weighed them down. On the other hand the superior classes, whose circumstances were different, their clothing more frequently changed, their persons more cleanly, their apartments less crowded and better ventilated, and among whom seclusion from the sick was practised, in proportion to their enjoyment of these advantages, generally escaped the disease."<sup>3</sup>

Dr. Bateman, after describing the methods to be adopted for promoting cleanliness and sufficient ventilation, remarks: "If these simple measures be steadily pursued, no confinement or accumula-

<sup>1</sup> Bateman on the Diseases of London, p. 18.

<sup>2</sup> Hancock on Pestilence, p. 297.

<sup>3</sup> Barker and Cheyne on Fever, vol. i. p. 134.

tion of morbid effluvia can take place under any state of fever; and the air of the apartment may be breathed, and the bed and person of the patient approached and touched with perfect impunity. If this were not the case, indeed, physicians and nurses, especially those employed in fever hospitals, would have little security for their lives. During the fourteen years, in the course of which I have almost daily been in contact with persons labouring under contagious fever, not only myself but all the nurses have thus been preserved from infection, with one exception, down to the period of the present epidemic." He adds in a note: "It is no disparagement to the system above described that some of the nurses and the matron of the House of Recovery have been infected during the present epidemic, which has kept the wards constantly full. The impossibility of maintaining a free ventilation night and day, during the cold weather, their perpetual exposure, in close contact, to the breath and discharges of the patients, while feeding, moving, or washing them, changing their beds and linen, and even stripping off their infected clothes on admission, might be sufficient to counteract the solitary operation of any general system, however efficacious. But the truth is, that the ventilation of the house has been very imperfect, and even at the command of the nurses and patients; and the injurious consequences of this imperfection have become so manifest, that the subject is now under the consideration of the committee, while this work is in the press."<sup>1</sup>

Dr. Hancock quotes the following facts, which illustrate very powerfully the influence of ventilation: "In the year 1819, I had occasion to see a very intelligent physician connected with one or two fever hospitals in Dublin, during the epidemic, who assured me he had seen no proof of the existence of contagion in the disease (typhus) as it appeared in those institutions under his care, where very great attention was paid to ventilation, and where the patients were not inconveniently crowded. But soon after this, I saw another physician no less intelligent, who informed me that in the course of about four months, between 200 and 300 persons were admitted into the Belfast Fever Hospital; and they were frequently so crowded in the wards as nearly to cover the floor with their beds; in which case, although the building is new, airy, and well regulated, the matron, twenty-two nurses, and the apothecary, took the disease; yet it was so mild, that scarcely more than one in fifty died."<sup>2</sup>

Dr. Prichard relates a striking example of the effects of a good as well as of a deficient ventilation, which occurred in two of the hospitals in Bristol, namely, St. Peter's and the Bristol Infirmary; both of these institutions being under his medical superintendence. "In the former, (St. Peter's,) the medical wards are very small, and it was necessary to place the beds very near to each other, and to

<sup>1</sup> Bateman on Contagious Fever, p. 154.

<sup>2</sup> Hancock on Pestilence, p. 339.

put too great a number of patients in a given space. Offensive smells were often perceptible; and it was under these circumstances that the disease was manifestly contagious." In the Bristol Infirmary the wards are lofty and well ventilated. "Here, also, the fever patients were dispersed among invalids of almost every other description. But no instance occurred of the propagation of fever; none of the nurses were attacked, nor were the patients lying in the adjacent beds in any instance infected, though cases of the worst description of typhus gravior were placed promiscuously among the other patients, scarcely two feet of space intervening between the beds."<sup>1</sup>

Drs. Barker and Cheyne state that "a remarkable proof was afforded, in Sir Patrick Dunn's Hospital, of a ward, by the peculiarity of its construction, protecting the attendants upon the sick from the effects of contagion. The ward alluded to is the fever ward for males, which extends the entire breadth of the left wing of the hospital, being sixty-two feet by thirty-eight. It is twenty feet high, and is subdivided by partitions, of the height of nine feet, into six apartments, two of which are thirty-eight feet by sixteen, and the rest are each nine feet square; the latter contains, with great convenience, four beds each, and the former ten; but on occasions of necessity, the square apartments have held five, and the oblong twelve beds without inconvenience; the partition walls leave two passages, one leading from the door of the wards across its breadth, and another passing in the middle of its length; it is furnished with three large fireplaces, two of which are in the oblong chambers, one on the north and the other on the south side of the ward, and the third opposite the door, at the end of the passage first described; by this door, the fever-ward opens on the staircase, which is walled and communicates with the corridors of the basement and underground stories. The greater number of the windows of the ward are sixteen feet from the floor, and in the ceiling are placed two louvres, one toward either end, by means of which and the fireplaces a brisk ventilation is kept up. During the late epidemic, when Sir Patrick Dunn's Hospital, by agreement with government, contained one hundred patients in fever, the male ward was crowded, containing forty-four patients, yet only one nurse was affected with fever; at the same period, the nurses in attendance on the female patients, who were certainly not so much crowded together, were continually taking the complaint, and generally had it with severity."<sup>2</sup> In addition to the facts which have now been brought forward, it may be stated, without much chance of contradiction, that, as there is in almost every large town deficient accommodation for fever patients in an hospital during an epidemic, overcrowding is an ordinary result, from the anxiety of the directors to relieve the misery of the sick. The Glasgow Fever Hospital is calculated to contain two hundred and twenty

<sup>1</sup> Hancock on Pestilence. Prichard's History of the Fever in Bristol, p. 88.

<sup>2</sup> Barker and Cheyne on Fever, vol. i. p. 483.



patients; and for nearly two years, namely, during 1836-7, it was generally filled to its maximum, and frequently from ten to twenty additional were accommodated. Now, it is quite obvious, that such a large number of fever patients, all contained in one building, will exhale a prodigious quantity of typhoid effluvium, which must be exceedingly concentrated; and that even the utmost cleanliness, and the greatest degree of ventilation consistent with the temperature that ought to be maintained, would scarcely be sufficient for its proper dilution.

Drs. Barker and Cheyne remark, in that portion of their report which has been already quoted, that typhus generally spreads in the families of the lower classes, and very rarely in those of the superior ranks. Dr. Cowan states that "the fever was chiefly, nay almost wholly, confined to the labouring classes, and to the districts which they inhabited, while among the wealthy and middle classes of society it was comparatively seldom met with; and when it did occur, was not spread by contagion through all the inmates of the family, as was usually the case among the families of the poor, but was confined to a single individual."<sup>1</sup> These results, as stated by the above-mentioned authors, agree, we are convinced, with those which have been made in almost every other place. This remarkable difference, in the two classes of persons referred to, must be owing chiefly to the wide diversity of circumstances in which they are placed; and approximates very closely to the difference which exists between a crowded and consequently an ill-ventilated hospital, and one which is limited to a small number of patients with thorough ventilation. The lower classes in large cities generally live in dirty, ill-ventilated houses, and are often filthy in their persons; while the better ranks live in more airy situations, have larger houses, and are more attentive to cleanliness in their persons and domestic habits; hence the effluvium which issues from a typhus patient, in the first-mentioned situations, cannot be carried off so readily, or diluted to the same extent with atmospheric air, as in the second. But it may be said by the opponents of typhoid contagion, that small-pox, measles, and scarlet fever, more frequently spread in the families of the better ranks than typhus; and why is ventilation and dilution not effectual in these cases? In answer to this objection, it may be stated, that these three last-mentioned diseases are not equally contagious, and that scarlet fever, particularly when it is not epidemic, is often confined to one person in a family; whereas small-pox, in the majority of instances, affects the greater number of unprotected persons, adults as well as children. M. Rayer states that "scarlatina is contagious, but to a less degree than measles. It affects chiefly children and young persons, more rarely adults. Every individual is not, to the same degree, apt to be affected with scarlatina, and every condition is not equally proper for its development. It attacks females more

<sup>1</sup> Cowan's Vital Statistics of Glasgow, p. 34.

readily than males; and some individuals, after having been exposed, in vain, during many days to the contagion of this disease, have been seized after the lapse of some time, in consequence of a simple communication with persons who had visited patients affected with this exanthema."<sup>1</sup>

Dr. Bateman states that adults are not very susceptible of the disease (scarlet fever), and that many medical practitioners who have attended great number of patients affected with it, have never experienced its effects.<sup>2</sup>

Dr. Mason Good observes that "nothing is more common than for a sporadic case of rosalia (scarlatina) to occur in a family without communication of itself to the surrounding children, although no pains may have been taken to keep them separate; while a few months afterwards it may possibly be received from a neighbour's house, merely by an accidental visit for a few minutes. In the one case, there was no predisposition in the habit to receive the complaint; in the other, the altered state of the atmosphere has, perhaps, produced such a predisposition in a very high degree, and prepared the way for the disease to become a very general epidemic. What this peculiar state of the atmosphere is, has not been very accurately ascertained."<sup>3</sup> Now, although it be granted that cleanliness and ventilation have somewhat less effect in preventing the spread of small-pox, measles, and scarlet fever, than in checking the progress of typhus; it has been shown by the above quotations that these diseases differ from one another in points equally material. It follows, therefore, that scarlet fever is regulated by a law similar to that of typhus, in being little contagious under certain circumstances; and that though cleanliness and ventilation may not prove an antidote equally efficacious to the contagion of small-pox, measles, and scarlet fever, as to that of typhus, yet to exclude the latter from the class of contagious fevers from this circumstance, would involve also the exclusion of scarlatina for an equally strong reason. In reasoning upon this subject, it does not seem difficult to conceive that one species of effluvium may be harmless, if diluted with a certain proportion of atmospheric air, while another may retain its virulency under similar circumstances; or that one species of effluvium may adhere with tenacity to every kind of clothing, while another is absorbed most readily by filthy garments, or by the deposits which are formed on the skin of an uncleanly person. We are in possession of no experiments which tend to prove such an opinion; but there is one analogy which will occur to every medical practitioner in vaccination. It is well known that if too much blood be drawn during the process of vaccination, the effect is very frequently prevented; and this is always explained on the principle, that the vaccine virus is diluted too

<sup>1</sup> Rayer des Maladies de la Peau, tom. i. p. 63.

<sup>2</sup> Bateman on Cutaneous Diseases, p. 70.

<sup>3</sup> Good's Study of Medicine by Cooper, vol. iii. p. 19.



much with the blood, as the same result follows when it is mixed with water. We have many analogies among the gases, such as carbonic acid, carburetted hydrogen, &c., to prove not only that when diluted to a certain extent with atmospheric air they may be respired with safety, but that each gas has its own peculiar law respecting the requisite proportion of dilution that is required for that purpose.

The majority of French physicians are of opinion that typhoid fever is not contagious, and this belief was almost universal until M. Bretonneau published a contrary opinion. The following quotation from M. Chomel will perhaps account, to a certain extent, for the opinions of the French physicians on this subject:—"Another circumstance contributes with us to render the transmission of a contagious malady difficult, particularly the typhoid disease, that in our hospitals every thing connected with cleanliness and ventilation is in the most perfect condition, and that the typhoid patients are never united, either in the same establishment or in the same ward, while their number is always very small, when compared with the number of those affected with other diseases; so that none of the conditions are present which favour contagion. It is the same with small-pox, and no one disputes its contagious character. In the wards of our hospitals, there are persons frequently affected with small-pox, and there are often individuals who have not been vaccinated, or who, not having undergone variola, are susceptible of contracting the disease; yet few instances of its transmission are evident. It is also very rare that the transmission of measles or scarlet fever from one subject to another, in the *Hôpital des Enfants de Paris*, can be verified, which even presents in some circumstances the most favourable conditions for the transmission of these diseases."<sup>1</sup> At page 98, some tables are given which tend to show the connection of filthy habits with typhus contagion.

2. *Typhus generally attacks individuals only once during their lives.*—The second law of contagious fevers is that they only affect persons once during their lives.

We believe this law to be completely established, and though there are instances of small-pox, measles, and scarlet fever affecting individuals more than once in their lives, yet these may fairly be considered only as exceptions to the general rule.

Before bringing forward the facts upon which the claims of typhus to be comprehended under this law may be founded, it is necessary to state that the evidence is by no means so clear and satisfactory as it is in small-pox, measles, and scarlet fever. These three last-mentioned diseases cannot, in the present day, be readily confounded with any other; for their diagnostic marks are very precise and definite; while the several kinds of continued fever have hitherto not been accurately ascertained, and have sometimes

<sup>1</sup> Chomel, *Leçons de Clinique Médicale*, tom. i. p. 321.

been considered merely as varieties of typhus ; hence the difficulty of establishing the application of this law to any one of them. We hope, however, it will appear from the quotations which shall presently be adduced, that the approximation of typhus to this law is so near as to preclude, in all fairness, its exclusion. M. Chomel, after remarking upon the number of persons that are seized more than once with pneumonia, states that "In the typhoid fever, on the contrary, notwithstanding the care with which the patients had always been interrogated on this point, no one, among one hundred and thirty persons who had been received at the *Clinique* affected with the disease, gave such a statement as could lead to the presumption that he had ever before laboured under it ; on the contrary, most of them asserted that it was the first time they had been ill."<sup>1</sup> He elsewhere adds : "We have already stated that the typhoid fever, in ordinary circumstances, affects the same individual only once. This is the result of all the facts hitherto collected. Since we began to make special researches and conclusions respecting this disease, no authentic example to the contrary has been observed, although the number of cases which are observed be very considerable, and examples of its return ought to be met with, if the malady were susceptible of being reproduced many times in the same person."

In interrogating our patients, we have always taken care to turn their attention from this quarter, but they have never answered in such a manner as to induce us to believe that they had laboured under the same disease ; and after all, though some contrary facts should be met with in so frequent a malady, these exceptions, which are little numerous, are nothing extraordinary, and do not overthrow the species of law which has been announced. Small-pox, scarlet fever, measles, which most generally attack the same individual only once, sometimes return, especially during epidemics of these diseases ; it will not be more astonishing if some examples of a return of the typhoid affection be met with. This circumstance is, then, a very important fact, for there are only a small number of diseases that attack the same individual only once, and amongst these maladies, there is none which is not evidently contagious ; the typhoid fever will, then, be the only exception to this kind of law, if it be not contagious like the diseases with which it has this important point of agreement. In the mean time we ought to observe that, though all the diseases which attack the same person only once are contagious, it does not follow that all those that are transmitted from one individual to another attack only once, many among them, as syphilis and the itch, are reproduced indefinitely."<sup>2</sup>

Dr. Lombard, of Geneva, when describing the difference between the continental and British typhus, states that "in one remarkable

<sup>1</sup> Chomel, *Leçons de Clinique Médicale*, tom. i. p. 309.

<sup>2</sup> *Ibid.* p. 333.



point, however, I believe they agree, I mean the fact that no one is known, or at least is very rarely known, to have the eruptive typhus twice. With us such instances are scarcely if ever met with, and I am informed that with you a person once attacked with typhus, attended with the measles like eruption, may safely calculate upon immunity from the disease for the future."<sup>1</sup>

Dr. Perry, of Glasgow, states as one of his conclusions respecting typhus fever, that "contagious typhus is an exanthematous disease, and, like smallpox, measles, and scarlet fever, during its course produces some change on the system, by which the individual having once undergone the disease is (as a general rule) secured against a second attack, and may with impunity expose himself to the contagion of typhus, if he continues to reside in the same country in which he previously had the disease. In those cases which are exceptions to the general rule, the disease appears in a mild and modified form, the crisis taking place on the seventh, ninth, or eleventh day." The same author states that this conclusion as well as the others in his paper are "the result of careful observation in upwards of 4000 cases."<sup>2</sup> Drs. Barker and Cheyne, who had the most extensive opportunities of ascertaining the history of typhus, seem to entertain opinions similar to those already quoted. They state that "at the hospital in Cork street, only one physician and the apothecary had an attack of fever; but then most of the physicians of the establishment had laboured under that disease on some former occasion previous to the appearance of the epidemic."<sup>3</sup> Dr. Cowan, as already quoted, states that all the gentlemen who have acted as clerks in the Fever Hospital for many years past have been attacked with fever unless they had it previously to their election.

Hildenbrand's opinion on this subject is of a more modified kind. He states that "the miasma of typhus, after having produced the fever, destroys almost always for a certain time the susceptibility to a similar contagion; nevertheless, it destroys it rarely for the whole of life, as does smallpox, measles, &c. It has, however, under this resemblance some analogy with the virus of these diseases, whilst on the contrary it totally differs from the syphilitic virus, which when once introduced into the human body, always favours more and more a similar contagion."<sup>4</sup>

The following table shows the answers to questions which were carefully put to patients who were admitted into the Glasgow Fever Hospital from November 1st, 1838, to November 1st, 1839. It includes the whole of the patients affected with eruptive typhus, from whom answers were obtained relative to any former affection with fever, as evidence from decided cases only could be made available in the elucidation of this point:

<sup>1</sup> Dublin Journal of Medical Science, vol. x. p. 22.

<sup>2</sup> Edinb. Med. and Surgical Journal, vol. xlv. p. 67.

<sup>3</sup> Barker and Cheyne on Fever, vol. i. p. 135.

<sup>4</sup> Hildenbrand, de Typhus contagieux, par J. C. Gasc, p. 118.

|                                 | Males. | Females. | Total.    |
|---------------------------------|--------|----------|-----------|
| Not previously affected . . . . | 284    | 251      | 535       |
| Previously affected . . . . .   | 33     | 41       | 74        |
|                                 |        |          | <hr/> 609 |

This table shows that out of 609 eruptive or decided cases of typhus there were only 74 persons who stated that they had previously laboured under fever. This part of the evidence may be reckoned positive; for individuals of all intellectual capacities remember a remarkable circumstance of this kind. On the other hand, the evidence respecting the nature of the former fever or affection is the converse of this; for only in a very few cases can it be correctly ascertained; and when we take into account the various diseases which are confounded with typhus (as shall be afterwards shown,) such as bronchitis, pneumonia, pleurisy, intestinal affections, febriculous or short fevers, and the numerous ailments of childhood, this small number can be satisfactorily accounted for.

It appears, therefore, that the evidence which can be produced to bear on this point, although not very extensive, decidedly supports the opinion that eruptive typhus fever affects individuals as a general rule only once in their lives; and it is to a considerable extent corroborative of this opinion, that almost all the clerks and nurses of the Glasgow Fever Hospital for the last six or seven years have had typhus characterised by the eruption, and not one of them, as far as we have been able to learn, have ever had it since; while almost all of them consider themselves now perfectly secure against a second attack, although constantly exposed to the effluvia arising from fever patients.

3. *Typhus is characterised by an exanthematous eruption.* The third characteristic of the contagious exanthemata is an eruption which has a regular rise, progress, and decline. The exanthematous eruption or rash which is peculiar to typhus fever has only been accurately attended to within these few years as a diagnostic symptom. It was, however, noticed by Rogers, in the fever which prevailed in Ireland during the year 1731, and one of the characteristic symptoms is described as "a universal efflorescence of petechiæ;"<sup>1</sup> also by Huxham in 1734-5, Sir John Pringle in 1750, &c.

No particular conclusion can be drawn from these authors' account of it; but when taken along with their general description of the disease, the opinion is corroborated that it was the very same affection that is so characterised at the present day. Hildenbrand described it in 1806 more particularly than any previous author. He states that it makes its appearance about the fourth day of the disease on the breast, loins, back, thighs, and arms, as being more warm, but sometimes on the face; that it is so much more abun-

<sup>1</sup> Barker and Cheyne on Fever, vol. i. p. 4.



dant as the eyes are red. He also remarks that petechiæ may exist with or without the eruption, are not indispensable phenomena, and are only developed in certain conditions. He farther observes that the *exantheme* is sometimes not present in those cases of typhus which are irregular in their progress.<sup>1</sup> The typhoid eruption was also a very general characteristic of the epidemic fever which prevailed in Ireland during the years 1817–18–19. Dr. Bracken makes the following statement: "Of about 250 cases which fell under my care in November and December of that year, the majority had eruptions of spots of various appearance as to size, shape, and colour. They were generally of a diffused appearance, gradually shading off, and insensibly disappearing, and of the size of a grain of hemp-seed, but sometimes much larger or much smaller. The distinct, well-defined petechiæ were frequently seen of a bright brown or purple colour. The shoulders seemed to be more frequently affected by these eruptions, but the whole surface of the body was often covered by them."<sup>2</sup> Drs. Barker and Cheyne give the following account of the eruption, as deduced from reports received from several parts of Munster: "As the disease advanced it was observed in most or all parts of the province that eruptions of different kinds, either closely allied to or varieties of those termed petechial, very generally accompanied it. In some instances the eruption was papular, or a motley appearance of the skin, or a rash somewhat resembling measles showed itself. At Cork, Dr. M. Barry remarked that in the species of fever which he terms synochus, petechiæ seldom occurred earlier than the fourth or fifth day; but his observation, if it does not express it directly, at least implies that their occurrence was frequent. They were generally of a bright red colour, sometimes small, at other times large. He did not consider them dangerous, nor find it necessary to abstain from those measures of depletion which were useful when high excitement prevailed. In a communication from Clonmell, Dr. Fitzgerald states that petechiæ occurred in four cases out of five. At Listowel, petechiæ was so common that Dr. O'Connell did not see six cases of fever unattended by a petechial eruption, which often appeared early in the disease." In the account of Connaught, the same authors state that "an early eruption of petechiæ, which were often to be observed on the third or fourth day or even earlier, and were visible for four or five days, was a general symptom of the disease; when petechiæ appeared thus early, they were not indicative of any malignancy." In the report for Ulster, it is stated that petechial eruptions were very common and that they occurred early. For Leinster, the same reporters state that one physician observed the petechiæ in seven cases out of ten, some thought them more general than they had been on any former occasion, and others represented them as universal. They appeared on the third,

<sup>1</sup> Hildenbrand, de Typhus contagieux, par J. C. Gasc, p. 53–4.

<sup>2</sup> Barker and Cheyne on Fever, vol. ii. p. 231.

fourth, or fifth days, continued visible for four or five days, and were often remarked in the mildest cases.<sup>1</sup>

The typhoid eruption, however, excited very little attention among the authors who wrote upon the epidemic that prevailed in Britain about the same period as in Ireland; and even up to a much later period it is only noticed in a cursory manner in our treatises on fever, and not as a diagnostic mark of great value. Dr. Alison in 1827 described it as a very frequent symptom of the epidemic which prevailed in Edinburgh about that period, occurring in a majority of the cases, and remarked that these eruptive fevers formed the connecting link between continued fever and the contagious exanthemata.<sup>2</sup>

M. Louis, who published his admirable work on gastro-enteritis or typhoid fever in 1829, states that "he has observed this eruption in twenty-six out of thirty-five cases, where it has been searched after, without saying that it was not present in some others; many of the persons in whom it was present had come to the hospital after the twenty-fourth day of the disease, at a period when the spots had perhaps disappeared."<sup>3</sup> M. Chomel gives the following excellent description of the typhoid eruption: "Usually from the seventh to the ninth day the eruption peculiar to typhoid fever makes its appearance, which consists in minute rose-red spots, disappearing on pressure from half a line to two lines in diameter, of a circular form, without elevation or scarcely raised above the level of the skin, dispersed over the abdomen, sometimes on the chest, less frequently on the thighs, the arms, and forearms. These little spots are so much the more distinct as the skin is white; in persons who have brown skins they are sometimes distinguished with difficulty. Their number cannot be determined because they are not all equally apparent; but in order to furnish a characteristic of the typhoid affection they ought at least to be from fifteen to twenty. When there are only two or three, no value can be attached to their presence. The eruption does not make its appearance on all points at once; often, after having noticed for three or four days some rose-coloured spots upon the abdomen, but in too small number to be considered as important, they are found all at once very numerous upon the chest and belly, sometimes upon the thighs, the arms, the back, and even the face, though very rarely. Its duration is not always the same; in some cases, after two or three days, there is no vestige of it; at other times it persists during twelve or fifteen: but in the latter case it consists of many successive eruptions: for each rose-coloured spot is usually visible for three or four days only, and sometimes less; and at the end of this time it disappears altogether, after having attained a colour less vivid. These spots present, at most, a slight elevation on the sur-

<sup>1</sup> Barker and Cheyne on Fever, vol. i. pp. 426, 454, 465, and 483.

<sup>2</sup> Edinb. Med. and Surgical Journal, vol. xxviii.

<sup>3</sup> Louis, de Gastro-Enterite, tom. ii. p. 231.



face of the skin, but they never have a conical form or vesicles at their apex. They rarely appear before the eighth day after the invasion of the disease. The following are the results of observations collected in our wards during the years 1830-1-2. Among seventy cases of typhoid fever, where the presence or absence of rose-coloured lenticular spots was carefully established, in sixteen cases, at no period of the disease could traces of this eruption be found; from which it may be inferred that in about one fourth of the persons seized with the typhoid affection this eruption is wanting."<sup>1</sup>

Chomel found that among fifty-four cases none presented the eruption before the sixth day, and in two it appeared after the thirty-sixth. This, he states, is confirmed by the observation of M. Louis, which were made on a much larger number of patients. He attaches great value to the eruption, as a diagnostic mark of typhoid fever, as it is as rare in other acute diseases as it is common in this.

Dr. Roupell states that in St. Bartholomew's Hospital, London, the eruption in typhus occurs in seventy out of every 100 cases.<sup>2</sup> Dr. West, in his account of the typhus exanthematicus as observed in St. Bartholomew's Hospital, states that "forty-two cases presented the peculiar measles-like eruption described by so many authors, which in all those cases in which I have been able accurately to note the date of its appearance, first showed itself from the sixth to the eighth day, generally on the former. It appeared in one instance on the fourth and another time on the fifth day; but I never saw it make its first appearance after the eighth day, though it was still visible on several patients admitted on the fourteenth, and on three who came to the hospital on the twenty-first day of the affection. Of the eighteen cases in which no eruption was observed, five only were admitted before the eighth day of the disease; it is, therefore, very probable that the eruption had existed in some of these patients but had disappeared before their admission."<sup>3</sup> Dr. Cowan has investigated the frequency of the eruption in the Glasgow Fever Hospital on upwards of 2000 cases, during the year 1835-6; and his results are the following: "At the close of the year, in 76·16 per cent. of the males, and 71·77 of the females, the typhoid eruption had occurred, giving as an average of the whole cases 73·99 out of every 100 admitted."<sup>4</sup>

Dr. Craigie found the typhoid eruption only in seventy-nine among 169 cases in the Edinburgh Royal Infirmary;<sup>5</sup> while Dr. Henderson discovered it in 108 cases out of 130 in the same institution at a subsequent period.<sup>6</sup>

<sup>1</sup> Chomel, *Leçons de Clinique Médicale*, tom. i. p. 18.

<sup>2</sup> Roupell on Typhus, p. 35, 1838.

<sup>3</sup> Edinburgh Medical and Surgical Journal, July, 1838, p. 140.

<sup>4</sup> Cowan's Vital Statistics of Glasgow, p. 26.

<sup>5</sup> Edinburgh Medical and Surgical Journal, vol. xxvii. p. 301.

<sup>6</sup> Ibid. October, 1839, p. 437.

In the Glasgow Fever Hospital, from May 1st to Nov. 1st, 1839, during which time the presence or absence of eruption was carefully noted, the proportion was as follows :

|                                    | Males. | Females. | Total.    |
|------------------------------------|--------|----------|-----------|
| Cases with Eruption . . . . .      | 224    | 217      | 441       |
| Cases without Eruption or doubtful | 130    | 120      | 250       |
|                                    |        |          | <hr/> 691 |

This table includes every case, with the exception of smallpox, measles, hooping-cough, and scarlet fever.

Dr. Peebles, in 1835, gave a very minute and excellent account of exanthematous typhus, and states, as the result of a minute inquiry into the subject in Great Britain and on the continent, that "he has found the eruption as constant as any exanthema of other eruptive diseases."<sup>1</sup>

It appears, therefore, that the eruption is present generally in from 70 to 75 out of every 100 patients that are admitted into fever hospitals in this country as well as in France. It must be well known, however, to every hospital physician, that cases are frequently admitted as continued fever, that are found, on examination, to be other diseases, and which are usually included in the total enumeration ; but this point shall be further illustrated in another part of the essay.

It is also well known that many cases of fever are admitted at a very late stage of the disease, as may be proved by statistical tables and also from the great number of deaths that occur on the first, second, and third days after admission ; hence it is extremely probable that the eruption has disappeared in a certain proportion of those who have the other decided symptoms of typhus. There is one fact, however, which powerfully supports the opinion that contagious typhus, in the great majority of cases, particularly in adults, is attended with the eruption, namely, that almost all the instances of fever which have occurred during the last six or seven years among the physicians, clerks, nurses, &c. of the Glasgow Fever Hospital, have been accompanied with this exanthema. We have made careful inquiries respecting this point, and have only heard of one or two exceptions amongst at least 100 cases. We do not, however, mean to maintain that typhus fever cannot exist without the presence of this eruption ; on the contrary, we have repeatedly attended families where the majority only of those affected were so characterised, and we have remarked this two or three times, when several of a family were sent into the hospital.

The want of regularity in the appearance of the eruption, and its persistency in regard to time, has been considered opposed in analogy to that of smallpox, measles, and scarlet fever ; and cer-

<sup>1</sup> Edinburgh Medical and Surgical Journal, vol. xliv. p. 373.



tainly, though these three diseases are more regular in their characteristic eruption than typhus, yet in scarlatina anginosa the eruption is frequently irregular or altogether absent. Dr. Tweedie, who must have treated scarlet fever extensively in the London Fever Hospital, makes the following statement: "Indeed, we are inclined, from our own experience, to affirm that the scarlatina simplex, scarlatina anginosa, and the scarlatina or angina maligna, and the sore throat without efflorescence on the skin, are merely varieties of one and the same disease."<sup>1</sup> He also quotes the results of Dr. Willan's experience during an epidemic scarlatina in the year 1786. "Of 251 cases, there were 152 of scarlatina anginosa, forty-two of sore throats without eruption on the skin, and thirty-nine of scarlatina maligna."<sup>2</sup> Rayer states that "often it does not appear until the third day, and is not dispersed so constantly upon the whole surface of the body. . . . . It is sometimes entirely effaced on the day of its appearance, and is developed anew at a period more or less near. . . . . The appearance of the exanthema is tardy; its tint is feeble and livid; it is interspersed with petechiæ, and its duration is uncertain. It appears and disappears many times."<sup>3</sup> It appears, therefore, that scarlet fever often differs from smallpox very materially in the regularity of its eruption; for that of the latter disease is extremely regular and almost unvarying in its rise, progress, and decline; while in the former it is frequently absent, and in other cases so evanescent as not to be distinctly recognised. The eruption characteristic of typhus again differs from that of scarlet fever, in being still less regular than it; but there is not a greater difference, if there be not less, in this respect, between scarlet fever and typhus than there is between scarlet fever and smallpox.

4. *Typhus cannot be checked in limine.* It has also been stated by authors, and it is a prevalent opinion among medical practitioners, that typhus can be checked in its early stages, and that in this respect its law is totally different from the exanthematous fevers. If this opinion were correct, the analogy between these affections and typhus would be greatly diminished, though not completely undermined; for it is not contrary to experience to suppose that some agent may be discovered that might be capable of modifying or destroying the poisonous principle that is lodged in the body. Those, however, who believe in the possibility of checking typhus *in limine*, have assumed a false premise, at least one which is not admitted, from which they draw their conclusions, namely, that every febrile affection which resembles this disease in its early symptoms is identical with it. Now it is well known to every medical practitioner that many of those febrile attacks which arise from disturbance of the digestive functions and from vicissitudes of temperature are attended with the same symptoms as

<sup>1</sup> Cyclopædia of Practical Medicine, vol. iii. p. 647.

<sup>2</sup> Ibid., p. 653.

<sup>3</sup> Rayer, Traité de Maladies de la Peau, tom. i. pp. 59, 60.

typhus in its early stages; and yet they will subside in a few days under every variety of treatment, and frequently without any treatment at all, at least such as could produce any effect on the system. If typhus fever, which is frequently so prevalent, could be checked in its progress by the means which are generally employed for that purpose, namely, bleeding, purging, sweating, &c., this doctrine would long ere now have been established with the same certainty as that peritonitis or pneumonia can be checked by a similar system of treatment; and yet the disease proceeds onwards in its course, unrestrained by the heroic but occasionally injudicious attempts to arrest it. If, then, there be febrile affections which subside in a few days under every variety of treatment, and often without any possessing a curative operation, it follows that those who make the assertion that they can check typhus *in limine*, should prove that their cases did not belong to the febriculæ we have referred to; or what would amount to the same thing, make their experiments upon an unequivocal example of the disease, namely, one characterised by the eruption, and demonstrate that they can stop its career.

*Crisis of typhus is pretty regular in cases not complicated.* A second objection has been brought forward against the inclusion of typhus among exanthematous fevers, namely, that it has no regular crisis like these last-mentioned diseases. This objection does not seem to have much weight attached to it; and to give it any degree of importance, it would be necessary to prove that all the other fevers of this order are uniformly characterised by a crisis on a particular day. Now, what are the facts connected with the history of this point in scarlet fever. Dr. Bateman states that the rash in scarlatina anginosa does not always appear on the second day, as in scarlatina simplex, but not unfrequently on the third; nor does it so constantly extend over the whole surface, but comes out in scattered patches, which seldom fail to appear about the elbows. Sometimes, too, it vanishes the day after its appearance, and reappears partially at uncertain times, but without any corresponding changes in the general disorder; the whole duration of the complaint is thus lengthened and the desquamation is less regular. . . . The same author, after describing the dangers which result from hemorrhage, diarrhœa, &c., in malignant scarlatina, states that "even those who escape through these dangers have often to struggle against many distressing symptoms for a considerable length of time, such as ulcerations spreading from the throat to the contiguous parts, suppuration of the glands, tedious cough and dyspnœa, excoriations about the nates, &c., with hectic fever."<sup>1</sup> When treating of measles, Rayer remarks that "it is never the exantheme which compromises life. The gravity of the evil depends upon the internal inflammation which accompany or succeed it.—The appearance of the measles before the third day, the sudden

<sup>1</sup> Bateman on Cutaneous Diseases, pp. 73 and 85.

disappearance or the leaden redness of the spots, the appearance of petechiæ, much difficulty of breathing, are severe symptoms. 'They are often characteristics of bronchitis and pneumonia, the existence of which is easily ascertained by auscultation and percussion of the chest. . . . . When the symptoms of gastro-pulmonary inflammations, which accompany the exantheme of measles, are little intense, and when it travels over its periods *easily and regularly*, the treatment of the disease is very simple.'<sup>1</sup> It is obvious from these quotations, and it is well known to every experienced practitioner, that the crisis of measles and scarlet fever varies considerably in different individuals, being pretty regular and early in the simple cases, and more or less protracted and irregular in those that are complicated with any organic affection. In typhus fever, uncomplicated with any serious organic disease of the head, chest, or abdomen, the crisis occurs very frequently about the same period in persons of a similar age; for young persons, as a general rule, pass through the disease more quickly than those more advanced in life.

Chomel states that the crisis or amelioration of the symptoms in sixty-eight cases, occurred in fifty, or in about three out of four, from the fifteenth to the thirtieth day.<sup>2</sup> Dr. Arthur Thomson states that the average duration of 2630 cases was twenty-seven days; and this calculation was made from cases described and enumerated in the works and papers of Drs. Bateman, Welsh, S. Smith, Latham, and Craigie.<sup>3</sup> There is certainly considerable irregularity as to the period when the crisis takes place in typhus in different individuals; at the same time, it may be remarked that a majority of patients begin to ameliorate within a certain period, and the reports from the various authors already quoted in regard to this point do not differ very materially as to the mean duration of fever, showing, even with imperfect statistics, a near approximation to some law by which it is regulated. It ought to be observed, however, that the evidence obtained from public hospitals is still very uncertain; for unless very careful and repeated inquiries be made to the patient, no satisfactory or accurate answer can be obtained, as to the period when the disease commenced; for he is often partially incoherent, and, in almost all cases, more or less confused in his ideas; and even though there be an opportunity of questioning his friends, more or less of cross-examination is generally required to elicit a correct answer.

A degree of uncertainty also arises from not calculating the crisis always at the same period. Some, as Chomel, Mills, Stoker, &c., calculating the termination of the disease from the commencement of the convalescent stage; while others have included the whole period of the patient's residence in the hospital in its duration. It is obvious that great discrepancy must arise from such a different

<sup>1</sup> Rayer, *Traité des Maladies de la Peau*, p. 24.

<sup>2</sup> Chomel, *Leçons de Clinique Médicale*, vol. i. p. 44.

<sup>3</sup> *Edinburgh Medical and Surgical Journal*, July, 1838, p. 109.



method of calculation; for a patient is often a week and sometimes two weeks in an hospital after the period of convalescence commences. Another uncertainty on this point has arisen from not classifying patients according to their different ages, such as is employed in calculating the mortality of typhus at the different periods of life; for if the disease be shorter in its duration, as it certainly is, in young persons than in those more advanced in life, it is impossible to expect uniformity by arranging the whole together. It would also contribute to elucidate this point, were the duration of the undoubted cases, namely, those characterised by the eruption, classified separately; as by this means, the duration of typhus would not be confounded with that of other continued fevers, and this method might also be made available as one of the means of diagnosis.

The very frequent complications of typhus with organic affections in the different cavities of the body is another reason amply sufficient to account for a considerable portion of its irregularity as to termination; and as these complications occur more frequently in this disease than in smallpox, measles, and scarlet fever, it follows that allowance should be made for its greater irregularity on these accounts; as it has already been shown by quotations from authors that some of the exanthematous fevers are also rendered irregular and protracted by organic complications.

Dr. Arthur Thomson gives the following table of the complications of fever compiled from cases related by Drs. Smith, Tweedie, Alison, and Craigie, and it shows that the complicated varieties are much more numerous than the simple or uncomplicated.

|   |       |
|---|-------|
| Simple fever . . . . .                      | 374   |
| Fever with cerebral complications . . . . . | 375   |
| thoracic      do. . . . .                   | 264   |
| abdominal   do. . . . .                     | 180   |
| mixed       do. . . . .                     | 308   |
|   | <hr/> |
|   | 1501  |

5. *Relapses in typhus do not occur after complete convalescence, unless some local disease be present.* The occurrence of relapses in fever is also brought forward as an important difference between it and the contagious exanthemata; and almost every author who has written on this subject mentions them as more or less frequent at different periods and during different epidemics. They do not seem to occur frequently in the typhoid fever, for Chomel does not make any particular reference to this point; and he merely states that the convalescence is sometimes prolonged, and that troublesome results are sometimes the consequence of satisfying hunger.

In considering this subject it is necessary, however, to observe that relapses, according to most authors, do not mean a return of the fever after complete convalescence, but a return of the symptoms with their former intensity, after a partial recovery, or what in the majority of cases might more properly be called a remission of the disease; for it is perfectly obvious that if the febrile symp-



It appears from these tables that among the cases of typhus there was not a single relapse into the same febrile state, characterised by a new eruption and the other distinctive marks of this disease; but on the contrary that all the secondary affections were well marked local diseases. It is also shown that two cases of febricula and one of intestinal fever were affected with typhus during their residence in the hospital; and it is probable that more of such cases would have been infected had not the precaution been adopted of dismissing them as early as possible.

In concluding our remarks upon relapses, we shall make the following quotation from Drs. Barker and Cheyne's work, in order to show that one of the most powerful facts in favour of the doctrine of relapses may be explained by the theory we have adopted. These authors state that "as the epidemic advanced and particularly in its latter stages, relapses became very common, insomuch that a very large proportion of those who had been attacked suffered a relapse, and with many this happened several times. . . . It was remarked at Roscrea that the more early the crises occurred the greater was the probability of relapse. This observation will apply to every part of this province, for as the epidemic fever approached to a close, a fever of short duration, continuing for about five days, extremely mild and rarely proving mortal, became very frequent, and at this time the tendency to relapse was most observable. On the contrary, after fever of long continuance, it rarely happened that relapse took place."<sup>1</sup> . . . . The same authors in their medical account of fever in Connaught, state that "relapses were so rare at the commencement of the epidemic that Dr. Veitch, Physician to the County Infirmary in Galway, in his letter of the 6th September, 1817, says that he had not observed one case of relapse out of some hundred cases of fever." In describing the disease as it occurred among the upper ranks in Galway, they state that "petechiæ were universal, insomuch that scarcely a case occurred without them."<sup>2</sup>

The inferences which may be deduced from these quotations are, 1st. That these short or five-day fevers were either not typhus or their convalescence was only a remission of the disease; for we are not aware of any writer on this subject who describes it as terminating so early. 2d. Very few of those which were protracted, or which continued to the end of the second or third week, relapsed, which is about the average period for the duration of typhus. 3d. That in Galway, where petechiæ or the typhoid eruption were almost universal, showing the disease to be typhus, not a single case of relapse occurred out of some hundred cases.

<sup>1</sup> Barker and Cheyne on Fever, vol. i. p. 438.

<sup>2</sup> Ibid. p. 455.



SECT. III.—*Sources of Continued Fevers, not Typhoid.*

Pneumonia, pleuritis, peritonis, bronchitis, and modifications of these affections are not unfrequently confounded with continued fever, being admitted to fever hospitals as such; and thus the numerical amount of non-eruptive cases of typhus is often considerably increased by the inclusion of these diseases in the list; independent altogether of the two other affections which we are about to describe, and which are generally considered continued fevers, although different from typhus in their prominent features and laws. The first and most prevalent of these two affections has been called *febricula*, on account of its mildness and short duration, when compared with typhus. The second is prominently accompanied with derangement of the digestive organs, either in the form of constipation or diarrhœa. Chomel makes the following observations, when treating of the diagnosis of typhoid fever: "In effect, various diseases may present, during the first three or four days, a great resemblance to the typhoid affection. Among the diverse morbid states which may at this period present analogous phenomena, we shall find the early symptoms of many eruptive diseases, as smallpox, scarlet fever, and measles; also some catarrhal affections of little intensity: protracted ephemeral fever may be taken for the typhoid inflammatory fever, bilious derangement for bilious fever, exhaustion for the commencement of an adynamic fever, and especially a latent phlegmasia either visceral or venous. . . . One of the most important characters of the typhoid affection is the duration of the febrile state. As often as the febrile phenomena which can be attached to any appreciable lesion are prolonged beyond a certain limit, eight or ten days for example, there will be already serious grounds for presuming an alteration of the glands of Peyer; and when a disease terminates at the end of some days, we can always be assured, whatever doubts may have existed as to its nature, that it was different from the typhoid affection; and thus all the morbid states, the duration of which does not extend to the tenth or twelfth day, are distinguished."<sup>1</sup>

1. *Sources of Febricula.* This affection generally commences, like typhus and several other febrile affections, with a rigor, attended by headach, frequency of pulse, heat of skin, flushed face, thirst, moist tongue, generally more or less coated with a whitish fur, and red at point and edges, more or less constipation of bowels, and in the great majority of cases uncombined with any determinate local affection. It is difficult to distinguish this fever from typhus for the first four or five days; but after that the diagnosis may in most cases be made with tolerable accuracy.

If the typhoid eruption be present, there can be no doubt whatever of the nature of the disease; for in Britain this peculiar

<sup>1</sup> Chomel, *Leçons de Clinique Médicale*, tom. i. p. 400.

efflorescence occurs in no other febrile affection that could be confounded with typhus ; but in a certain proportion of cases it is not present in the latter disease. In cases of typhus destitute of the eruption, there are frequently, however, other symptoms present, even by the sixth day, which are rarely if at all observed in febricula ; such as suffused eyes, delirium, or partial stupor, a dry and brown tongue, a dark or dusky hue of the skin. The frequency of the pulse is also a very important symptom in the diagnosis ; for in febricula it is rarely above 100, and it generally continues full or of moderate strength throughout the whole course of the disease ; whereas in many cases of typhus, the pulse becomes weak, soft, small, or very compressible, at an early period of the disease, and in most cases is more or less above 100 about the sixth or seventh day. Sometimes this fever terminates in one or two days, being described by some authors under the name of ephemera ; but more generally symptoms of amendment appear about the sixth or seventh day, and complete convalescence is established, in the large majority of cases, from the sixth to the tenth day. Deafness and desquamation of the cuticle, both of which are frequent characteristics of typhus, are generally absent in this affection. Again, complete convalescence from typhus rarely occurs in adults before the fifteenth day, and is in a majority of cases much later. In children, however, the crisis of typhus generally appears earlier than in adults ; but the febriculous affections to which they are liable are proportionally short, often only one or two days in duration. The statistical facts connected with the minimum and maximum duration of typhus have not been very conclusively determined ; for, as we formerly remarked, one class of authors terminate the disease when the stage of convalescence begins, while another class do not consider it terminated until the patient is discharged from the hospital ; and this discrepancy is still further increased by not carefully classifying the different febrile affections that are admitted into fever institutions and their corresponding duration.

M. Chomel, who seems to have been exceedingly careful in drawing his conclusions only from decided cases of typhoid fever, gives the following statistical account of the duration of the disease, from its commencement to the beginning of convalescence :

|   |   |              |                   |            |
|---|---|--------------|-------------------|------------|
| In 1 patient on the 8th day after attack. |   |              |                   |            |
| 1   | " | "            | 9th               | "          |
| 4   | " | "            | 12th              | "          |
| 3   | " | between      | 12th and 14th day | inclusive. |
| 10  | " | "            | 15th and 16th     | "          |
| 15  | " | "            | 17th and 20th     | "          |
| 14  | " | "            | 21st and 25th     | "          |
| 11  | " | "            | 26th and 30th     | "          |
| 8   | " | "            | 31st and 40th     | "          |
| 1   | " | on the 45th. |                   |            |

"If, however," he adds, "we throw aside the cases in which improvement has appeared before the fifteenth day, and those in which it has appeared after the thirtieth, which constitute a small number of exceptions, there remains fifty cases out of sixty-eight, that is nearly three fourths, in which this improvement took place, from the fifteenth to the thirtieth day." It appears from this table that there were only two out of sixty-eight cases that presented symptoms of convalescence at the eighth and ninth day, and if we add five or six days for its complete establishment, the disease, even in this fractional proportion of cases, could not be considered as terminated before the thirteenth or fourteenth day.

This method of calculating the duration of fever, adopted by Chomel and many other authors, is greatly inferior in accuracy to that of marking the patient convalescent when he is actually free of the febrile symptoms, namely, when his pulse is natural, his tongue pretty clean, his sleep tolerably sound, and his appetite moderately good, but still weak, and consequently unable to leave the hospital for some days at least. It is quite obvious that the positive character of these four symptoms renders them more fixed, more easily ascertained, and not so likely to be misapprehended as their relative improvement during the first stage of convalescence; and therefore that it is preferable in the determination of this question.

It appears necessary, before presenting our table constructed on this principle, to give one which will show the whole diseases that have been admitted within a certain period into the Glasgow Fever Hospital, namely, from May 1st to November 1st, 1839; as some of our deductions depend upon a fair and impartial consideration of these cases, and as the various statistical points referred to in this section were noted with care.

<sup>1</sup> Chomel, *Leçons de Clinique Médicale*, tom. i. p. 44.



| Continued<br>Fever. |                                 | Males. | Females. | Total. |
|---------------------|---------------------------------|--------|----------|--------|
|                     |                                 |        |          |        |
| {                   | Typhus, . . . . .               | 270    | 276      | 546    |
|                     | Febricula, . . . . .            | 32     | 31       | 63     |
|                     | Gastric or Intestinal Fever, .  | 8      | 7        | 15     |
|                     | Bronchitis, . . . . .           | 14     | 8        | 22     |
|                     | Pneumonia, . . . . .            | 15     | 7        | 22     |
|                     | Smallpox, . . . . .             | 16     | 11       | 27     |
|                     | Measles, . . . . .              | 3      | 1        | 4      |
|                     | Scarlet Fever, . . . . .        |        | 4        | 4      |
|                     | Whooping-cough, . . . . .       | 1      | 1        | 2      |
|                     | Hydrocephalus, . . . . .        | 1      |          | 1      |
|                     | Erysipelas, . . . . .           |        | 3        | 3      |
|                     | Roseola, . . . . .              |        | 2        | 2      |
|                     | Erythema, . . . . .             | 1      |          | 1      |
|                     | Hepatitis, . . . . .            | 1      |          | 1      |
|                     | Apoplexy, . . . . .             | 1      | 1        | 2      |
|                     | Determination of blood to head, | 1      |          | 1      |
|                     | Intermittent Fever, . . . . .   | 1      |          | 1      |
|                     | Cynanche Tonsillaris, . . . .   | 1      | 1        | 2      |
|                     | Syphilis, . . . . .             | 1      | 1        | 2      |
|                     | Delirium Tremens, . . . . .     | 1      |          | 1      |
|                     | Suppuration of Kidneys, . . .   |        | 1        | 1      |
|                     | Phthisis, . . . . .             |        | 2        | 2      |
|                     | Dysentery, . . . . .            |        | 2        | 2      |
|                     | Mania, . . . . .                |        | 1        | 1      |
|                     |                                 | 368    | 360      | 728    |

As a considerable number of the cases in the above table were not continued fevers, it may be necessary to explain one or two points respecting the admissions into the Glasgow Fever Hospital. The facilities of admission have of late been very great, in consequence of there being much more accommodation than was required; and every case, where there was the slightest suspicion of fever, seems to have been sent to this institution, not only from the city, but from its vicinity to the extent of many miles.

It may be supposed that there is a large number classified as bronchitis and pneumonia; but it requires to be stated that in all the cases of the first mentioned disease, there were no typhoid symptoms present, and that only two or three were arranged under this division, whose convalescence extended beyond the tenth day, while the greater number of the pneumonic patients were bled from the arm, and the blood found decidedly buffy.

The case marked suppuration of the kidneys was one of peculiar interest. The patient had been delivered of a child about a fortnight before her admission, and was at this latter period found quite comatose, but there were none of the peculiar symptoms of typhus present. The inspection, however, cleared up any doubt that existed as to the nature of the affection, for both kidneys contained numerous small abscesses throughout their whole texture,

there was pus in both pelves, in the ureters, and bladders, but no urine in the latter organ.

We shall next present a table, showing the maximum of the pulse and the period of complete convalescence in 181 cases of eruptive typhus, and in thirty cases of febricula, that were admitted into the Glasgow Fever Hospital from May 1st to November 1st, 1839, and it includes the whole that were admitted within that period, except two or three, whose convalescence and pulse were not noted, and those that were omitted for reasons to be presently stated.

*Table of the Maximum Frequency of the Pulse in 181 Cases of Eruptive Typhus.*

| MALES.                                     |               | FEMALES.                    |               |
|--|---------------|-----------------------------|---------------|
| Maximum frequency of pulse.                | No. of Cases. | Maximum frequency of pulse. | No. of Cases. |
| 86   | 5             | 96                          | 12            |
| 96   | 20            | 98                          | 1             |
| 100  | 8             | 100                         | 3             |
| 104  | 4             | 104                         | 5             |
| 106  | 3             | 108                         | 23            |
| 108  | 15            | 110                         | 1             |
| 110  | 1             | 112                         | 3             |
| 112  | 4             | 116                         | 3             |
| 116  | 4             | 120                         | 17            |
| 118  | 1             | 124                         | 7             |
| 120  | 18            | 130                         | 10            |
| 124  | 5             | 134                         | 2             |
| 128  | 1             | 140                         | 4             |
| 130  | 1             |                             |               |
|  | <hr/> 90      |                             | <hr/> 91=181  |
| Average maximum of pulse in Males = 107.5. |               |                             |               |
| " " Females = 114.1.                       |               |                             |               |
| " " Males and Females = 110.8.             |               |                             |               |

The five cases in which the pulse is marked 86 were admitted on the seventh, ninth, eleventh, fourteenth, and twenty-first days of the disease, so that it is probable that partial convalescence had commenced at the time the pulse was noted.

*Table showing the Day of the Disease on which complete Convalescence was established in 181 cases of Eruptive Typhus.*

| MALES.   |               | FEMALES.                          |               |
|--|---------------|-----------------------------------|---------------|
| Day of disease.  | No. of Cases. | Day of disease.                   | No. of Cases. |
| 12th   | 1             | 13th                              | 2             |
| 13   | 4             | 14                                | 7             |
| 14   | 2             | 15                                | 11            |
| 15   | 9             | 16                                | 3             |
| 16   | 9             | 17                                | 9             |
| 17   | 9             | 18                                | 10            |
| 18   | 6             | 19                                | 6             |
| 19   | 7             | 20                                | 10            |
| 20   | 3             | 21                                | 3             |
| 21   | 10            | 22                                | 5             |
| 22   | 8             | 23                                | 2             |
| 23   | 2             | 24                                | 3             |
| 24   | 6             | 25                                | 1             |
| 25   | 2             | 27                                | 4             |
| 26   | 4             | 28                                | 1             |
| 27   | 4             | 29                                | 3             |
| 28   | 1             | 30                                | 2             |
| 29   | 3             | 32                                | 1             |
|  |               | 34                                | 4             |
|  |               | 36                                | 1             |
|  |               | 44                                | 1             |
|  |               | 54                                | 2             |
|  | 90            |                                   | 91=181 tot.   |
| Average convalescence in Males = 19.7 days. <sup>1</sup> |               |                                   |               |
| "  | "             | Females = 21.3 days.              |               |
| "  | "             | days in Males and Females = 20.5. |               |

Every case below twenty years of age has been excluded, because the maximum of the pulse varies more from childhood to adolescence than during any other similar period of life; and those who died have also been excluded, as the comparison between the pulse and the recovery would not be uniform in the two diseases, and as the average maximum of the pulse of those cases which terminated fatally was greater than that of those who recovered.

We have taken the eruptive cases of typhus only by which to illustrate the law of convalescence and frequency of the pulse; in order to prevent any doubt as to the nature of the fever from which the conclusion is drawn, and because they constitute the large majority of fever patients. But it may be said that though the non-

<sup>1</sup> Dr. Henderson states that he has seen instances of convalescence on the seventh and eighth days, in which the eruption had existed; but it is not mentioned at what stage of convalescence the calculation was made, and what were the ages of the patients. *Edinburgh Med. and Surg. Journal*, Oct. 1839, p. 430.



eruptive cases constitute a small or perhaps only an exceptional proportion of the whole number, they may not follow the same law as the majority, but may be milder, and that the severity of the cases is in proportion to the amount of eruption. Dr. Henderson supports this opinion, which is founded on the examination of about 200 cases in the Edinburgh Infirmary. We can so far support the author of this paper in regard to the general severity of the cases attended with a copious eruption; but certainly there is no uniform proportion between the two, for we have frequently met with mild cases of typhus in which there existed a copious eruption, and occasionally with some which terminated fatally when there were only a small number of spots. Indeed, reasoning by analogy from scarlet fever, to which typhus has most resemblance in the irregularity of its eruption, we should be led to infer that the intensity of the symptoms would not probably correspond uniformly with the copiousness of the eruption; for cases of scarlet fever have often been found very malignant during some epidemics, although not characterised by any exanthematous eruption, or by one which was only extremely indistinct or evanescent.

There seems to be, therefore, no valid reason why the law of typhus respecting complete convalescence and the frequency of the pulse should not be deduced from the eruptive cases, as they constitute, at least, about three fourths of the whole number, and as there is no uniform proportion between the amount of the eruption and the severity of the symptoms.

*Table of the Maximum Frequency of the Pulse in 30 cases of Febricula.*

| MALES.  |               | FEMALES.                    |               |
|---|---------------|-----------------------------|---------------|
| Maximum frequency of pulse.                           | No. of Cases. | Maximum frequency of pulse. | No. of Cases. |
| 68  | 2             | 72                          | 1             |
| 72  | 7             | 74                          | 1             |
| 76  | 1             | 76                          | 1             |
| 82  | 1             | 84                          | 3             |
| 84  | 1             | 88                          | 1             |
| 86  | 1             | 90                          | 2             |
| 92  | 1             | 92                          | 2             |
|   |               | 96                          | 1             |
|   |               | 100                         | 3             |
|   |               | 104                         | 1             |
|   | —             |                             | —             |
|   | 14            |                             | 16=30 tot.    |
| Average maximum of pulse = 82.8 in Males and Females. |               |                             |               |

*Table, showing the Day of Disease on which complete convalescence was established in 30 Cases of Febricula.*

| MALES.   |               | FEMALES.        |               |
|--|---------------|-----------------|---------------|
| Day of disease.                                      | No. of Cases. | Day of disease. | No. of Cases. |
| 4th  | 1             | 3d              | 1             |
| 7  | 3             | 4               | 1             |
| 8  | 3             | 5               | 1             |
| 9  | 2             | 6               | 1             |
| 10   | 5             | 7               | 2             |
|  |               | 8               | 3             |
|  |               | 9               | 3             |
|  |               | 10              | 4             |
|  | —             |                 | —             |
|  | 14            |                 | 16=30 tot.    |
| Average convalescence = 8 days in Males and Females. |               |                 |               |

These tables show that, in 181 cases of eruptive typhus occurring in adults, the maximum frequency of the pulse was not below 96, except in five cases; that in about three fourths it was 108 and upwards, and that the average maximum of the whole was 110.8. They also show that only one case of typhus was convalescent on the twelfth, and six on the thirteenth day of the disease out of this number; and that the average convalescence of the whole was 20.5 days. Contrast this with febricula, in which out of thirty cases the pulse did not exceed 100, except in one patient, in whom it was 104; and the average maximum of the pulse for the whole was only 82.8. The convalescence in any of these cases of febricula did not exceed the tenth day, and their average convalescence was eight days. Are there, then, reasons for maintaining the opinion that these short and mild fevers are specifically different from typhus, in opposition to that of Bateman and many eminent authors? We think there are; for if diseases are to be discriminated by a difference of laws and phenomena, there is certainly in these two affections a wide distinction in their symptoms, and also a distinct line of separation between them as regards the period of their duration and the frequency of the pulse.

This view is supported by the fact that febriculous patients have been frequently affected with typhus during their convalescence in the Glasgow Fever Hospital, which cannot be satisfactorily explained on any other principle than that these two affections are different in their nature. The causes which are generally assigned for febricula also tend to support its disjunction from typhus; for although they have not been sufficiently investigated, yet there is an approximation to something like a proof, that exposure to cold is more frequently an antecedent to this affection than it is to typhus. The following table shows the causes that were assigned for the following cases of febricula:

| Cold. | Uncertain. | Contagion. | Total. |
|-------|------------|------------|--------|
| 22    | 28         | 10 =       | 60     |

This result tends to support the popular belief and that of many medical practitioners, that there is a short fever which has sometimes been called "a cold fever," although not necessarily attended by a cough or other pectoral complaint.

It is not probable that this affection is contagious, for though more than one in a family sometimes become affected, this is not generally the case, as in typhus among the lower classes; and it is rare that more than one person from the same house has been admitted for this disease into the Fever Hospital. Besides, the fact formerly stated respecting the almost uniformly typhoid and exanthematous character of the disease in the Glasgow Fever Hospital, when nurses and hospital attendants became affected, has a tendency to support this belief; for cases of febricula are always found associated with typhus in every institution of this kind, when there is no particular restriction respecting admission; and if it were contagious, it is probable that some of the attendants would have been affected with it. And though these short and mild fevers are not generally described and classified separately as to their phenomena and laws, there is abundant evidence existing in the writings of our British and Irish authors to prove that they constitute a greater or less proportion of the fever cases of Great Britain and Ireland. It does not appear to be confined, like typhus when not epidemic, to particular localities, such as large towns, &c., and in all probability it is the most common sporadic fever met with in many country districts. It seems also to be capable of attacking the same individual more than once during his life; and we have in a number of instances attended the same individual within a few years under two different attacks, both having the same characteristics of mildness and shortness. If this view be adopted, it may account to a certain extent for the statements that typhus fever has often been known to affect a person more than once during his life; the one fever being confounded with the other.

We have no facts sufficiently conclusive to bring forward respecting its mortality; but undoubtedly it is very small, unless complicated with a particular local affection: and when a disease which originally has all the characters of febricula becomes protracted, the diagnosis becomes so obscure that any deductions drawn from it are very questionable.

If the analysis of the cases admitted into the Glasgow Fever Hospital during the six months already specified be granted, it will tend to reduce the number of those without eruption very considerably. It is stated at page 26 that there were 250 cases without eruption, and 441 in whom this exantheme was observed. Now among these 250 cases there were 145 other affections than typhus, which, being deducted from 250, leave as those really non-eruptive 105, being above 80 per cent. of eruptive cases. But the number of those cases without eruption might be still farther reduced; for a portion of them were admitted after the tenth day of the disease, when it is presumable that the exantheme might have disappeared.



and some of them were verging on convalescence; so that even during the non-epidemic prevalence of typhus, when other febrile affections bear to it a larger proportion than when it is extensively diffused, the number without eruption is not very great; and this fact may account for the opinion which is held by some authors and by many medical practitioners that the exantheme is chiefly characteristic of typhus during the prevalence of an epidemic.

2. *Sources of Gastric or Intestinal Fever.*—This febrile affection is very often of an ephemeral kind, lasting only two or three days, and hence it is not frequently met with in hospital practice. Sometimes it results from excesses in eating and drinking, which have been repeated in rapid succession; occasionally it is caused by a single indulgence in some aliment difficult of digestion. Persons who have feeble or dyspeptic digestive organs, particularly if the bowels be constipated, are very liable to this affection if their habits be irregular. The person attacked generally feels a kind of malaise for some days previous to the rigour which often ushers in the febrile symptoms; the pulse is sometimes extremely rapid, the skin hot, the tongue is coated with a thick white fur, and there is frequently nausea and an uneasy feeling in the abdomen, which is more or less tumid. The bowels are always either constipated or there is diarrhœa, and when the latter symptom is present, even when the stools are feculent, there is very generally reason to suspect, at least at the commencement of the disease, the existence of solid excrementitious matter in the cells of the colon. This affection is sometimes suddenly terminated by a copious perspiration; but more generally, not until the bowels have been freely unloaded of their feculent contents; and we have repeatedly met with cases of obstinate constipation, in which the febrile symptoms did not completely subside for six or eight days.

In many cases it may be distinguished from typhus at the commencement by ascertaining the antecedent circumstances of the patient, and by the state of his bowels and abdomen. When the diagnosis is doubtful during the progress of the affection, its short duration in the great majority of cases must necessarily distinguish it from typhus. In some instances, however, particularly when diarrhœa is present, the attack is prolonged for a week or two, and sometimes for two or three weeks. In some of these cases there is a tendency to peritonitis, while in others there is reason to suspect some enlargement or ulceration of the glands of the intestines. We are quite aware that such cases, which are not of frequent occurrence, might be called typhus fever without eruption; and in the present state of our diagnostic means this question cannot be solved in a satisfactory manner; but we hope that future investigators will be able to define a line by which they may be distinguished. That intestinal fevers, even those of a protracted nature, are specifically different from typhus may be deduced from the fact that repeated instances have occurred of such patients being affected with eruptive typhus during their convalescence in the fever hos-

pital, and a case of this kind is mentioned in the table of secondary diseases at page 31. Dr. Lombard, of Geneva, in a recent publication, maintains the opinion that there is a bilious fever which is quite distinct from the typhoid fever; but at the same time acknowledges the extreme difficulty of the diagnosis. He states that "the facts collected justify the inference that there are insensible degrees between a simple 'embarras gastrique' and the most severe typhoid fever; but it does not thence follow that there are no true bilious diseases and no true gastric derangements, because we have cited cases of this kind which have terminated by death without presenting any of the lesions characteristic of typhoid fever; only it appears very difficult to distinguish if a mild case of gastric derangement arises from a simple derangement of the alimentary canal, or if, as in a case related of a suicide, it is accompanied by a developement of the glands of Peyer. Perhaps in the lenticular eruption may be found the distinctive sign of the intestinal eruption and of the bilious disease. But farther observations are necessary to determine this in at all a satisfactory manner."<sup>1</sup>

Various other forms of fever than those we have described have been mentioned by authors; but we have seen no reason to believe, either from the account given of them or from our own experience, that there are any other species.

The typhoid eruption has been found in almost the whole of those that were formerly considered distinct fevers; and has identified into the same species, synochus, typhus mitior and gravior, adynamic, ataxic, putrid, spotted, and jail fevers; while synocha or inflammatory fever is admitted to have scarcely an existence in this country, and it is not very easy to conceive how inflammation could exist without the presence of some local inflammatory action.

3. *Bronchitis*.—Bronchitis is a frequent complication of typhus fever; but this inflammatory affection is also confounded with it and other continued fevers when there is the strongest evidence for believing that the febrile symptoms are solely dependent upon the bronchial inflammation. It may be distinguished from typhus by the affection of the bronchi being almost uniformly the first symptoms of the disease, as indicated by hoarseness, cough, dyspnœa; whereas the bronchitic symptoms in fever are rarely present to any extent at the very commencement. The febrile symptoms in bronchitis are almost always proportionate to the greater or less severity of the bronchitic inflammation, increase as it increases, and decline when it is diminishing, which latter result is often well indicated by the expectoration of yellowish opaque mucus. The duration of bronchitis is also generally shorter than that of typhus, unless it be complicated with some pneumonic inflammation; and when this occurs there may be some difficulty in determining the case. If, however, there be the distinct stethoscopic signs of pneumonia, if the blood be decidedly buffy, and not simply coated with a whitish

<sup>1</sup> Clinical Remarks on Bilious and Typhoid Fevers, p. 16

or greenish-white pellicle, if the skin be of its natural whiteness, if the febrile symptoms be proportionate to the local affection, if there be no stupor, delirium, or suffusion of the eyes; even although the typhoid eruption be not present, there will be a tolerable certainty that the disease is not typhus fever. The effects of a full bleeding are not to be overlooked; for in pure pneumonia, its influence in reducing the frequency of the pulse and the urgency of the other symptoms is generally very decided, which is by no means the result when typhus is associated with this disease.

I. *Alleged sources of continued fevers from putrid effluvia.* It is a well-established fact that the accidental inoculation of the body with decayed or putrid animal matter has produced morbid symptoms, resembling in some respects those of typhus fever, and many medical men have been so affected, after making necroscopic inspections. There is always, however, in such cases extensive local disease of the member inoculated, or a diffused cellular inflammation. According to the researches and experiments of MM. Gaspard, Magendie and Leuret, and Hamont, putrid animal matter, when injected into the veins of healthy animals, proves speedily fatal,<sup>1</sup> and putrid vegetable matter acts similarly, though to a less degree; while the symptoms induced have some resemblance to those in typhus fever.

The following were the symptoms which were produced in a dog, into the jugular vein of which M. Gaspard injected a putrid solution of fermented cabbage, on the 14th July, 1821. Some hours after the injection of the liquid, there was great malaise, difficult respiration, vomiting, and great weakness. At the end of nine hours a very copious black and liquid stool. On the 15th, the weakness was more considerable; there was lateral decubitus, small and feeble pulse, ardent thirst, natural and abundant urine, free respiration, strong pulsations of the heart, as in aneurism with hypertrophy of that organ. On the 16th, some improvement, less weakness, no pulsations of the heart, great thirst, disinclination to food, fever, and occasionally vomiting of drinks; 17th, the same symptoms; 18th, symptoms aggravated, extreme feebleness, staggering locomotion, excessive thirst, red inflamed eyes and filled with mucus, tumefied nostrils obstructed with mucus, mucous membrane of mouth red and phlogosed, a liquid grayish-white stool with some clots of putrid blood, and death at the end of the fifth day of the experiment. On dissection, the lungs were found black and slightly inflamed, but still sufficiently crepitant. The right ventricle of the heart contained an albumino-fibrous concretion, which extended into the superior cava and pulmonary artery. The mucous membrane of the intestines, especially that of the duodenum and rectum, and a portion of the small intestines was violet-red, as if ecchymosed, inflamed chiefly in the form of longitudinal wrinkles and by irregular plates, which variegated the

<sup>1</sup> Christison on Poisons, p. 583.



exterior of the intestines before their incision. The mucous glands of the rectum were swollen and very distinct. The mesenteric glands appeared to be engorged with blood and were completely inflamed, the gall-bladder was filled with black, thick, and ropy bile.<sup>1</sup>

In several particulars the symptoms of a malignant case of typhus were exemplified in this experiment upon the dog; the small quick pulse, the peculiar decubitus indicating great weakness, the black stools, the red colour of the mucous membrane of the mouth and fauces, the injected eyes, and finally the staggering as indicative of delirium. The necroscopic inspection also furnishes some points of resemblance, namely, the inflammatory patches in the mucous membrane of the intestines, the enlarged glands in the rectum, the swollen and engorged mesenteric glands, the black ropy bile; all of which are pathological appearances more or less frequently met with in typhus. M. Magendie found that fatal effects were produced by confining dogs over vessels in which animal matters were undergoing the process of putrefaction; but pigeons, rabbits, and Indian hogs were not in the least injured by a residence in the same cage for nearly a month. He repeated many times this experiment with dogs, and always obtained the same result with one exception; but he states that in this case the dog was acclimated, for the injection of a putrid liquid into his veins had little effect upon him. The symptoms, however, are different from those produced by the injection of a putrid fluid into the veins; for the animals seem to die only from extenuation at the end of about ten days; and the post-mortem appearances are a total absence of fat, of aliments in the stomach, and of chyle in the lacteals; while the mucous membrane of the intestines is inflamed, but less so than when putrid matter is injected into the veins.<sup>2</sup> It appears, however, well authenticated that workmen employed in peculiar manufactories, and who are constantly exposed to the effluvia arising from animal substances in a state of putrefaction, are not subject to any of those morbid effects which result from the injection of putrid matter into the veins, or, according to M. Magendie, to those which result from exposure to putrid effluvia; there must, therefore, be some other explanation given of the last-mentioned author's experiments, or some unknown concurring circumstances must be required to bring the poison into operation. One of the most remarkable and repulsive manufactories or rather nuisances of this kind is the Chantiers d'Ecarrissage de la Ville de Paris. It is an inclosure of many acres of ground, situated close to the walls of Paris, and has existed for several centuries. Into this receptacle are carried the contents of the necessities of the city; and the carcasses of 40,000 or 50,000 horses, dogs and cats are flayed and cut up there annually. Various parts of these animals are separated and manufactured for sale: the intestines into gut for

<sup>1</sup> Journal de Physiologie, tom. ii. p. 16.

<sup>2</sup> Ibid., tom. iii. p. 85.

machinery; the fat is melted for blow-pipe lamps; the flesh, blood, &c. are collected for manure; a compost is made to breed maggots for feeding poultry, and the bones are chiefly used as fuel. Hordes of rats live in this bed of filth and extend their ravages extensively in the neighbourhood. The fetor which arises from it is overpowering, and often spreads to a great distance. It is remarkable, however, and contrary to every preconceived notion that could be formed respecting its salubrity, that the workmen of this establishment and their families are healthy, the most of them being stout and long-lived. This fact has been established satisfactorily by Parent-Duchatelet. This author states that they have all the characteristics of the most blooming health, that in this respect they resemble butchers, and that they seem to attain longevity more frequently than other artisans. Even new workmen employed upon extra occasions, although not acclimated, do not appear to be more susceptible, nor do they become affected with any disease. During the time that cholera prevailed in France, not an *écarrisseur* was affected with the disease, and not one was sick; and the mortality of the village which is in the vicinity of Montfaucon was very small when compared with that of Paris. He also quotes the innocuous influence of the human bodies which are exhumed to the extent of 200 annually from Père la Chaise, and the exhumations from the Cemetery des Innocents, amounting to about 20,000 bodies annually, which occupied three years in the execution, and which was also carried on during the greatest heats of summer.<sup>1</sup> Dissecting rooms are also situations where putrid effluvia are constantly present; and it has been affirmed that those who are much confined to these places do not enjoy good health, and are subject to fevers. MM. D'Arcet and Parent-Duchatelet state that the most frequent indisposition among those who are engaged in dissections is dyspepsia and diarrhœa, but that this latter affection is frequent among the strangers who arrive at Paris. These authors cite an immense number of authorities of the highest respectability, namely, Boyer, Dupuytren, Lallemand, Roux, Jadelot, Breschet, &c. to prove that dissecting rooms are not insalubrious and are not productive of fevers. M. Andral states that gastro-enterite, meningitis, and typhoid fever are common among the young *élèves* of medicine during the first year of their residence at Paris; but so little does this depend upon their sojourn in the dissecting amphitheatre, that among those who are affected, there is at least as many seized before they commence their dissections as after this period. He adds that the health of the men employed in handling the *débris* of dead bodies is similar to that of other individuals.<sup>2</sup> The workmen employed in the manufacture of strings for musical instruments are exposed constantly to the putrid effluvia of animal substances, arising from their long maceration, and they are not more subject to fevers than other tradesmen.

<sup>1</sup> *Annales d'Hygiène Publique*, tom. viii. p. 139. <sup>2</sup> *Ibid.*, tom. v. p. 301.

Butchers, who are believed by some authors to be almost exempt from fevers, are exposed in the slaughter-house to the emanations arising from the putrid blood and other animal fluids, which are frequently allowed to stagnate, and which are sufficiently indicated by the fetid and insupportable odour which issues from these places during hot weather. The atmosphere of whale vessels must be constantly impregnated or rather saturated with the effluvium that issues from large and numerous fishes; yet fevers are not prevalent among the seamen. Magendie states that the most deleterious animal poison is the putrid water of fishes: when some drops of this water are injected into the veins, in less than half an hour symptoms very similar to those existing in typhus and yellow fever are produced, and the animal dies in about twenty-four hours.<sup>1</sup> It appears from these facts that persons may live constantly amidst the most concentrated putrid animal emanations and yet not contract fever of any type; may enjoy health of the most perfect kind; attain longevity in many instances, and be less subject to some epidemic diseases than the inhabitants in their neighbourhood. It may be asked how are the experiments of M. Magendie and others to be explained upon this view? It does not appear from M. Magendie's experiments that the same symptoms or pathological appearances were produced by exposing dogs to putrid animal emanations, as by injection of a putrid fluid into the veins; indeed, he admits this himself; but adheres to the belief that the effluvium was the cause of death in the dogs subjected to experiment, although no injurious effects were produced on several other animals. Many animal poisons, however, operate differently on different organs and tissues; and this is well exemplified in an experiment mentioned by Dr. Christison, namely, that "a pupil of Professor Mangili swallowed at once the whole poison of four vipers without suffering inconvenience;"<sup>2</sup> but if a small quantity of this be inserted into a wound, poisonous effects are always produced. From a consideration of the whole evidence that might be adduced respecting this point, it may be drawn as a conclusion that although putrid matters, when injected into the veins of animals, cause death under symptoms similar to those of typhus fever, yet that the effluvia arising from similar matters do not under ordinary circumstances produce any deleterious effects on man. That there are exceptions to this general law we doubt not, such as Olivier being affected with diarrhœa after visiting a cellar filled with old bones, and Chevallier being seized with the same disease after exposure to the emanations from dead bodies; but that the effluvia arising from animal substances in a state of putrefaction constitute any regular source of continued fevers, we think there are no grounds for believing.

## II. *Alleged sources of continued fevers from the exhalations of*

<sup>1</sup> Journal de Physiologie, tom. iii. p. 83.

<sup>2</sup> Christison on Poisons, 3d Ed. p. 577.



*the human body.* Another modification of putrid miasmata has been noticed by almost all authors as a cause of fever, namely, the concentrated exhalations from the human body. Sir John Pringle and other army as well as navy physicians have remarked that fever was often produced in crowded hospitals, especially during hot weather, and also in crowded barracks and in transport ships, when filled beyond a due number. Dr. Tweedie makes the following statement: "The late Mr. John Pearson told me that when he was surgeon of the Lock Hospital, he uniformly observed when more than a certain number of patients were placed in any of the wards, fever became prevalent in the establishment; and that from repeated observation of this fact, he was induced to restrict the number of beds in each ward, and never afterwards witnessed the recurrence of fever in the house."<sup>1</sup> Dr. Bateman remarks that "if it had not been already demonstrated, on the most copious evidence, that the mere accumulation of animal matter in a putrescent state is incapable of generating fever; yet the fact that the closeness of the habitations of the poor, the uncleanness of their persons, furniture, and apparel, and the accumulating filth in the lanes and alleys which they occupy remain unchanged in all seasons, while epidemic fever appears but rarely and with long intervals of absence, is decisive against the supposition that the latter is engendered from such sources."<sup>2</sup> It is singular that a writer of such distinguished accuracy, after having drawn so fair a conclusion from the facts connected with the prevalence of fever, should apparently contradict this; for he states in a note that "the morbid and even natural effluvia of the living body, when allowed to accumulate by want of cleanliness and air, are unquestionably common sources of fever, and contribute mainly to its propagations as has been intimated in the preceding note."<sup>2</sup> The inhabitants of some countries, such as the natives of Kamstchatka, are remarkable for their filth and for living amidst the most foul and putrid effluvia; and yet fever is not known among them. The places they live in are called *yourts*, which "are sunk seven or eight feet below the surface of the ground, and are covered with a thatched roof in the form of a truncated cone, open at the top; they consist of one small apartment, which usually contains six families, with their utensils and stock of provisions for the winter, the chief part of which is dried fish almost putrefied. . . . Here they eat, drink, and sleep, crowded promiscuously together, and satisfy all the calls of nature without modesty or restraint, and never complain of the noxious odour that prevails in these habitations."<sup>3</sup>

The same mode of living is practised by the inhabitants of the Island of Oonalaska, by the Samoiedes, by the Greenlanders and Esquimaux; and there are no continued fevers among them,

<sup>1</sup> Tweedie's *Clinical Illustrations of Fever*, p. 83.

<sup>2</sup> Bateman on *Fever*, pp. 5, 6, and 7.

<sup>3</sup> Bancroft on *Yellow Fever*, &c. p. 121.

although scurvy prevails to a considerable extent. In many parts of Russia the same system of living in filthy and unventilated houses, and in an atmosphere saturated with human effluvia, is practised; yet no febrile disease is the result. Dr. Bancroft quotes the slave-ships as examples, where an atmosphere is more offensively impregnated with human exhalations, excretions, &c. than could probably be found in any other place of confinement, and makes the following statement: "I am fully convinced that fever of any kind rarely occurs on board these vessels, and contagious fever never; though great mortality has frequently happened from other diseases, and more especially from dysentery. . . . ."

There certainly is nothing in the constitutions of the negroes which exempts them from typhus or contagious fever; on the contrary they have been found as susceptible of it as whites, and considerable numbers of them who were sent from this country and from Nova Scotia to the new colony of Sierra Leone died of it on their passage thither, as will be more fully related in another place."<sup>1</sup>

Dr. Bancroft quotes a very remarkable instance of crowding in the *Decade* frigate during the revolution in France; where 193 persons were crowded to as great a degree as the negroes are in slave-ships, yet not one of them died during a period of ninety-six days.

III. *Alleged sources of jail fever from filth and an impure air.* The breaking out of fever in jails has often been brought forward as a proof of the origin of the disease from filth and impure atmosphere, being afterwards propagated by contagion; and Sir John Pringle's aphorism is frequently quoted or alluded to by writers on fever, namely, that "the cause seems plainly to arise from a corruption of the air pent up and deprived of its elastic parts by the respiration of a multitude, or more particularly vitiated with the perspirable matter, which, as it is the most volatile part of the humours, is also the most putrescent."

Dr. Bancroft makes the following very pertinent remarks upon this point: "That this fever often exists in them (jails) cannot be denied; but this circumstance can afford no evidence of its having been generated therein any more than the multiplication of vermin in such places could demonstrate the spontaneous generation of these and other insects by the nastiness which favours the deposition and hatching of their eggs."<sup>2</sup> . . . . Indeed, if it were true that the vegetable or animal matters while decomposing or putrefying could, *de novo*, generate contagion properly so called, the species or varieties of contagion ought necessarily to have become as numerous and various as the matters so decomposing, and also as various as their relative proportions; every dunghill, every collection of rubbish and filth, ought to be capable of generating the cause of a new disease, and that disease ought to be capable of reproducing itself in other persons."<sup>3</sup> In estimating the value of

<sup>1</sup> Bancroft on Yellow Fever, &c. p. 129.    <sup>2</sup> Ibid. p. 149.    <sup>3</sup> Ibid. p. 105.

the testimony that is generally brought forward to prove the spontaneous origin of fever in jails, and the great improbability of the first person attacked, who may have been resident there for several months, being infected previously to his imprisonment, the remarks which we formerly made respecting the impossibility of tracing the contagion in similar situations, even of diseases universally admitted to arise from that cause alone, will also apply here. And although jails may apparently be the most secure places against the inroads of contagion, from the number of their bars and gates; yet their inmates, from the nature of their offences and their dependent situation, must have a more frequent communication with their friends, either personally or through the medium of clothes, than is generally supposed, and that too frequently with the most filthy and debased of the human race. The question relative to the spontaneous origin of fever in jails seems to be almost solved by the fact that the same cause existing in a variety of other situations produce no disease like continued fever; and it can be proved from the history of other prisons, namely, those in Switzerland, Italy, Russia, &c., and similarly circumstanced as to filth, want of ventilation, &c., that no such diseases were known there. Mr. Howard, who has investigated this subject in his work on prisons, states: "If it were asked what is the cause of the jail fever, it would, in general, be readily replied, the want of fresh air and cleanliness; but as I have found in some prisons abroad, cells and dungeons as offensive and dirty as any I have observed in this country, where, however, this distemper was unknown, I am obliged to look out for some additional cause for its production."<sup>1</sup>

The following fact is worthy of being quoted, as illustrative of the efficacy of cleanliness and disinfection of suspected clothes, &c. in preventing the introduction of fever into jails, which were formerly so much infected by this disease: "In the jail at Cork, the prisoners remained free from fever when it had spread in every direction among the inhabitants of the city. To prevent its introduction, means were employed which deserve record: jail dresses were provided for the prisoners, whose clothes on their admission were removed and heated in a stove, and their persons washed and cleaned; the bedding was occasionally steeped in oxymuriatic acid water and then stoved; patients in whom fever showed itself were immediately removed to an hospital; this system was continued during a year and a half, in the course of which time two prisoners died of dysentery but none of fever; when the medical inspector for Munster made his visit to the jail, the system had for some time been discontinued in consequence of the expense attending the jail dresses, and then fever began to show itself among the prisoners, and a few cases were found in the jail at that time."<sup>2</sup>

From a review of the whole facts connected with filth and

<sup>1</sup> Bancroft on Yellow Fever, &c. p. 149.

<sup>2</sup> Barker and Cheyne on Fever, vol. i. p. 97.



deficient ventilation, it appears that both in the countries where continued fevers prevail and in those where they do not exist, the inhabitants may live constantly amidst this impurity and yet be entirely exempt from any febrile disease of this kind; and that if filth and an impure air were a common source of fever in jails, hospitals, &c., without the influence of contagion, they would produce the same effects in all other countries and localities similarly situated and circumstanced. The opinion, therefore, which is so generally admitted and propagated by many of our first authorities, that fever may arise from common causes, such as putrid miasmata, contaminated air, &c., and yet afterwards be propagated by contagion, receives no support from this presumed source; for though we are not prepared to assert that febrile affections may not, under peculiar circumstances, arise from these causes; yet it is undoubtedly deducible from the evidence that they are not ordinary or even limited though regular sources of the disease in any form.

IV. *Alleged sources of continued fevers from river malaria.* Before concluding this part of the essay, we shall notice an hypothesis which has lately been somewhat confidently brought forward to account for the prevalence of typhus in some large cities, namely, that a peculiar malaria is generated by the animal and vegetable filth which accumulates along the sides of rivers running through large towns, and that the inhabitants who live in their immediate vicinity become thereby subject to fever. We are quite aware that very disagreeable and sometimes fetid effluvia occasionally arise from such situations, particularly during hot weather; but that it is capable of causing continued fever has not even been rendered probable by any satisfactory evidence.

We presume that this point may be determined by the locality of Glasgow; for the Clyde runs through the town, and has a numerous population inhabiting houses close to its banks. This river is also of considerable magnitude; and certainly there is abundance of filth deposited in its bed by the numerous common sewers and public works in Glasgow. We have kept a record of the places of habitation of 934 persons who were admitted into the Glasgow Fever Hospital from January 1st to November 1st, 1839, and have classified the cases in the manner shown in the following table. The town has been divided into seven districts: 1, includes all the streets parallel to the river and close to its banks on both sides; 2, all the streets on both sides of the river, which run at right angles to it and which open into it—these first two divisions are of course excluded from the others; 3, east district of the town, from the cross eastward; 4, west district, from Buchanan street westward, and bounded on the north by Sauchie-hall road; 5, north side, northward of Sauchie-hall road and Rotten row; 6, south side of the river, with the exception of those streets close to its banks or which open into it; 7, centre of the town, from the Cross to Buchanan street, and bounded on the north by Rotten row.

|  | Males. | Females. | Total.    |
|--|--------|----------|-----------|
| Streets close and parallel to river .                          | 14     | 10       | 24        |
| Streets at right angles to and opening<br>into river . . . . . | 56     | 51       | 107       |
| East district of town . . . .                                  | 140    | 136      | 276       |
| West district of town . . . .                                  | 44     | 54       | 98        |
| North side of town . . . . .                                   | 44     | 41       | 85        |
| South side of town . . . . .                                   | 41     | 22       | 63        |
| Centre of town . . . . .                                       | 92     | 106      | 198       |
| From the country, 2 to 14 miles .                              | 46     | 37       | 83        |
|  |        |          | <hr/> 934 |

It is shown by this table that among 934 cases admitted as labouring under continued fever, there were only twenty-four who inhabited houses close to the river; and when we take into calculation the large population that live upon its banks, this proportion is very small. Again, in those streets which run at right angles to the river, and which open into it, the number is greater; but it must be remembered that most of these streets are long, and that it is only those inhabitants who live at their river termination that are at all exposed to the effluvia. If this be taken into the account, a fractional proportion only of these 107 cases ought to be calculated. The east district of the town, a situation very remote from the river, furnished nearly one third of the whole cases, and it and the centre of the town together more than the half. These facts clearly show that river malaria has no influence in the production of continued fevers in Glasgow, and that it is proportionally as prevalent, if not more so, in other and more central parts of the town.

## CHAPTER II.

### Circumstances favouring the Diffusion of Continued Fevers.

Epidemic diseases have prevailed from the most remote era of the world, and have, with one exception, hitherto bid comparative defiance to the philosophy of medicine, in its attempts to check their progress or diminish their mortality. Almost every country has its own peculiar pestilence, that sweeps rapidly away its redundant population at periodical seasons; and then its fatal operation ceases, partly from the subjects it can attack being reduced in number, and partly because its laws rendered it progressive from one city or country to another, or because the element that favoured its operation had been changed or modified in its constitution. Many

diseases, the contagious as well as the non-contagious, possess the property of becoming epidemic; and smallpox, which is perhaps the most infectious of all febrile affections, is subject to the same law, being rapidly diffusible during some particular seasons, while in others it remains comparatively inactive. The same periodical prevalence of scarlet fever and measles is observed during particular seasons, as of yellow fever, which is generally believed to be a non-contagious disease, and to derive its origin from vegetable malaria generated in a hot climate.

There are four prominent circumstances which favour the diffusion of contagious continued fever :

1st. A humid state of the atmosphere.

2d. Poverty, famine, or food of bad quality.

3d. An accumulation of persons not previously affected.

4th. Filth and deficient ventilation.

We do not mean, however, to assert that these are the only circumstances that operate in the diffusion of contagious fever; for certainly our knowledge of the constitution of the atmosphere, particularly its meteorological and miasmatic qualities, warrants no such conclusion. But if it can be shown that these circumstances generally precede or accompany an epidemic of typhus fever; although they may not account for all the phenomena connected with its extension, yet may so far elucidate the subject as to facilitate the progress of future observations or experiments.

1. *Humidity of the atmosphere, scarcity of provisions, filth, and deficient ventilation, tend to diffuse continued fevers.* Almost all authors who have written on epidemic diseases have noticed what is called an epidemic constitution of the atmosphere; but this has in general been so indefinitely stated, far less defined, that no conclusion can be drawn from their descriptions as to the peculiar alterations of which it consists. We shall, therefore, confine our evidence solely to that state of the atmosphere which is either cognisable by our senses or by instruments, as it is impossible in the present state of our knowledge to advance any thing but vague hypothesis, upon what authors designate by the term referred to. Typhus fever is a disease peculiar to cold or temperate regions, and it does not appear that it is capable of propagation to any extent in a hot climate; the powerful heat of the sun in such regions appearing to dissipate or destroy its contagious properties. Dr. Bancroft observes that "in voyages to the East Indies, ships remain for a much longer space of time between the tropics, and being also exposed to a higher temperature, the power of heat in destroying typhus fever is in them more decisively manifested, an entire cessation of the disease (however prevalent) commonly taking place before they can reach the Cape of Good Hope. It has indeed never been known, as I am informed, that a single case of this fever had occurred on either side of the Indian peninsula." The existence

<sup>1</sup> Bancroft on Yellow Fever, &c. p. 510.



of typhus fever, at least its diffusion, seems therefore incompatible with a powerful or tropical heat of the sun, and in that respect it differs very essentially from yellow fever; but the ordinary heat of a temperate climate does not extinguish it or even materially check its progress; for it has often prevailed epidemically during summer as well as winter; though it has been generally observed that the seasons during its prevalence were attended with more than the average quantity of rain. In adducing evidence to prove this point, we shall also include that which establishes the coexistence of scarcity of provisions with its consequences, namely, filth and deficient ventilation, as the descriptions of authors generally comprehend these concomitant circumstances. Drs. Barker and Cheyne state on the authority of Rogers, that "after the year 1721, there was again an interval of good health in Ireland so complete, that scarcely a case of fever was to be met with; this continued till the year 1728, when, as we learn from Boulter's Letters, there had been three bad harvests in succession. Oatmeal, the chief food of the poor in the north, rose to an extravagant price; in the south, the scarcity was so severely felt, that on the 26th of February there was a great rising of the populace of Cork, who threatened to pull down the Mayor's house. . . . From 1728 fever gained ground, and continued to be epidemical until 1732. . . . In the winter of 1739-40 an intense frost attended with a high wind at S. E. and E., intolerably piercing, set in on the 27th of December, and continued with little interruption till the middle of February." The following season was one of great scarcity, and in "the autumn of 1740, which was unusually frosty, with a continued prevalence of N. and E. winds, fever which had been frequent became epidemical; it did not cease in the winter, and increased most alarmingly in the spring and summer of 1741." O'Connell estimated the mortality of that epidemic at 80,000 persons. "The year 1800 was nearly as unfavourable to the fruits of the earth as 1799. The summer of that year was unusually dry; then followed a short period of uncommon heat; for three weeks or a month the thermometer, when at its greatest height during the day, seldom fell below 70 degrees; cold and wet weather set in about the end of August or beginning of September. Thus a short period of uncommon heat degenerated into an ungenial autumn, yielding in some soils an imperfect produce, whilst in others, the failure of the crops was little less complete than in the preceding season; so that, notwithstanding bounties were granted on the importation of foreign corn, and the distillation of spirits from grain prohibited, yet the price of bread and potatoes, both of bad quality, together with that of every other necessary of life, was raised beyond all precedent. In the autumn and winter of 1800, the inhabitants of this kingdom universally suffered from a contagious fever, in which the troops still continue to participate. . . . In August, 1801, the garrison of Dublin suffered greatly from petechial fever, which very generally prevailed among all ranks in the metropolis

and its vicinity. The epidemic which had now reached its height shortly after began to decline, but not before the good effects of an unusually abundant harvest, in again furnishing provisions of all kinds to the poor at a moderate rate, had been felt. The winter of 1813-14 had been uncommonly severe, that of 1815-16 did not fall short in severity; but particularly so in the early part of 1816, when the cold was very great in these countries. In the month of February, 1816, the quicksilver in the thermometer in many parts of England fell below  $0^{\circ}$ . Thus at Northampton, on the 9th of February, 1816, it fell to  $4^{\circ}$ , and on the 10th to  $2.75^{\circ}$ . In the neighbourhood of London it fell to  $5^{\circ}$  below  $0^{\circ}$ , and during four days of that month it never rose to the freezing point. . . . . From a registry of the weather kept in Dublin, it appears that the mean temperature of the months of spring, summer, and autumn, commencing with February and ending with October of that year, was nearly three degrees and a half below that of the similar preceding period; thus the medium temperature in 1815 was  $54.32^{\circ}$ , and during the same time in 1816 it was only  $50.9^{\circ}$ , the difference amounting to  $3.42^{\circ}$ . In neighbouring countries similar observations were made. According to those of Mr. Howard, in the neighbourhood of London, the mean temperature of the same months in 1815 was  $53.9^{\circ}$ , and in 1816 only  $49.9^{\circ}$ , the difference amounting to four degrees. . . . . The quantity of rain which fell during the summer and autumn of 1816 was also very great. During the months commencing with July and ending with October in that year, being the season of harvest, the humidity of the atmosphere was almost incessant: rain falling during the greater part of the time in these months. . . . . The effects occasioned by unusual cold and humidity, and absence of sunshine on the productions of the soil were peculiarly injurious. The harvest of grain was uncommonly late both in this country and in England. Corn remained uncut during the latter parts of October and November, and much of it was altogether lost. The same injurious effects on the quality of the potatoes were produced as upon the grain, and these roots constitute "the principal or only food of the poor" in most parts of Ireland. "The sufferings of the poor at this period did not depend on diminution of vegetable food only; in many or most parts of Ireland, the straw used for bedding was often half decayed, and more than usually disposed to imbibe and retain humidity; perhaps from deficiency of the woody fibres. . . . . Turf or peat is the chief fuel of the poor in this country, and during such wet seasons it could not be cut and dried for use. So great was the scarcity of fuel, that the hedges, which in ordinary times are respected as the boundaries of property, were destroyed, and the trees in many places were denuded of their branches to supply the necessities of life; a practice at which landed proprietors often connived, sensible that it had arisen from necessity the most urgent. Hence dampness of clothes and bedding, imperfect cooking of food and ventilation of apartments, deficient cleanliness in persons and

dwellings, all depending on the want of fuel, contributed to heighten and extend the calamities of the poor of Ireland at this eventful period. The preceding statement refers to the effects of the cold and wet of 1816 chiefly, but the following year was little inferior in severity. The summer and autumn were humid, cold, and ungenial, and agricultural produce, with the exception of potatoes, which were more abundant than in the former year, was almost as scarce as in 1816. . . . The year 1818 was remarkable for a state of weather the reverse of that in the years immediately preceding. The spring was moist, but the summer set in with unusual warmth, and proved the hottest which has occurred in this country during many years past." To these causes of distress were added a very low price for labour, and extensive failures in trade and manufactures. Drs. Barker and Cheyne also mention the prevalence of fever on the continent of Europe during a series of previous years, and remark very justly that "the circumstances of the inhabitants of a great portion of the continent at this time, arising from the distress occasioned by its being the seat of war, must have strongly resembled those of the people of Ireland during the late scarcity of provisions. At a later period, in 1817, after a failure of the crops, epidemic fever existed in the southern parts of Italy. .

. . . . From the same authority (Dr. Pockles) we learned that in the early part of 1817, scarcity of food was so great in Germany, that many died of hunger; but no epidemic fever existed there at that time. It had prevailed in that country three years previously, and did not then originate in scarcity of provisions, but was traced to the miserable remnant of the French army which entered that country after its overthrow in Russia. From the facts here adduced, it follows incontrovertibly, that during the times of its increase in Ireland, fever was very prevalent in most parts of the continent, and that the circumstances which caused it to spread epidemically were not peculiar to this island. . . . But whatever may have been the causes which have rendered the disease more than usually frequent during the last nine or ten years, no distinct evidence has been obtained of its introduction from the continent; and an inspection of the preceding table (vol. i. p. 49) points out that the rapid increase of the disease depended on general causes, operating on most parts of the country at the same time. For we find that it commenced in places situated most distant from each other in different parts of Munster and Ulster, at the end of 1816 or beginning of 1817; and making the proper allowance for the difficulty of determining when fever became epidemical in places which are always infested by the disease, we must admit that the periods of its manifest increase were nearly coincident. In fact, the scarcity of provisions combined with want of employment, whatsoever their mode of operating may have been, appears as the main cause of the spreading of fever epidemically through this country; although it must also be acknowledged that the simultaneous increase of this disease in Ireland and on the continent, leads



to the inference that whatever may have been its origin an epidemic constitution prevailed over a great part of Europe during a series of past years. . . . With respect to the time of its greatest prevalence in each of the four provinces it is not easy to decide. In Munster, it appears to have been most prevalent in the summer of 1818, and in Connaught about the same time, whilst the other provinces, where its commencement was latest, the time of its greatest prevalence was referred in Leinster generally to the autumn of 1817, and in Ulster to the winter of that year. In the principal cities, Dublin, Cork, and Limerick, it was most prevalent in the summer and autumn of 1818."<sup>1</sup>

We have thus made very copious extracts from Drs. Barker and Cheyne's valuable record of the epidemic fever in Ireland; as it contains a greater amount of facts and observations respecting this disease than any work that we have consulted. Indeed, when we consider that about one fourth, or a million and a half, of the population of Ireland were affected with fever, during the two years that it prevailed, and that accurate communications were received from respectable physicians residing in all the provinces, and that these have been admirably concentrated and illustrated by the authors, it must be considered one of the most important as well as interesting descriptions of the rise, progress, and decline of this disease.

Dr. Adams states that "during the winter of scarcity in 1799 and 1800, fever from infectious atmosphere was so general as to excite us to imitate the example of those manufacturing towns which are never free from the disease, and a fever-house was established in London."<sup>2</sup> Dr. Bateman remarks that "deficiency of nutriment is the principal source of epidemic fever, and that the circumstances just alluded to (improvement in all the arts of life,) operate only as accessories in fostering and multiplying it will scarcely admit of dispute. . . . The last epidemic which occurred in London followed a scarcity of two successive years (1799 and 1800); and it was during the prevalence of this fever that the necessity for establishing a House of Recovery became manifest. . . . Whether the epidemic of 1817 has been really much more extensive than the former, I am unable to determine. . . . It might have been expected, indeed, that the present epidemic would exceed the last in the extent of its course, since it occurred at a period of unparalleled distress among the labouring poor; when the loss of employment, occasioned by the termination of the war and the general suspension of the manufactures, concurred with the failing harvest of 1816 to increase the difficulties of procuring subsistence."<sup>3</sup> Dr. Tweedie observes that "it is an undeniable fact, founded on the experience of many epidemics, that there are certain circumstances which render the system peculiarly

<sup>1</sup> Barker and Cheyne on Fever, pp. 25-107.

<sup>2</sup> Adam's Inquiry into the Laws of Epidemics, p. 30.

<sup>3</sup> Bateman on Contagious Fever, pp. 4 to 11.

predisposed to the action of febrile causes; and the connection of scarcity and privation with the occurrence of fever among the lower classes of the community, has been so often verified by the experience of epidemics, as now to be received as a general axiom."<sup>1</sup> The same author also makes the following observations on the influence of the temperature and moisture of the atmosphere: "Though fever can scarcely be said to have prevailed extensively, or to adopt the common phrase, to have been epidemic in London since 1820, yet the diminution of autumnal fevers, for the last two seasons, proves decidedly how much some unknown condition of the atmosphere influences its prevalence. This condition is intimately connected with the combined effects of heat and moisture; hence cold and wet summers are always remarked to be comparatively healthy, while disorders of the bowels in such seasons are seldom observed. The number of patients admitted into the Fever Hospital in the autumn months of the last three years establish this principle. In August, September, and October, 1827, there were admitted 205; in the same months of 1828, the numbers were 170; in the autumn of 1829, only 94 were received. The cause of this progressive diminution is undoubtedly to be traced to the cold wet summers of the last two seasons."<sup>2</sup> An opinion, exactly opposed to that of Dr. Tweedie, is given by Dr. Armstrong. He states that "in England typhus is evidently favoured by a low temperature, being most prevalent in the cold seasons of winter and spring, generally abating or disappearing as the heat of summer advances, and often prevailing to a considerable degree in cold wet autumns."<sup>3</sup> Dr. Alison makes the following statement respecting the cause of the epidemic fever which prevailed in Edinburgh during the years 1826-7: "The chief cause of the unusually great and rapid extension of fever during last winter was no doubt the very distressed condition of a great part of the lower order of inhabitants, in consequence of the diminished expenditure of the higher ranks, and particularly of the failure of many speculations in building, which had given employment to great numbers of masons, joiners, plasterers, and labourers. . . . A very great number of the patients received into the hospitals in fever belonged to families of which the working members had been out of employment for periods varying from six weeks to six months; and Edinburgh has furnished but too many opportunities, both recently and formerly, for observing that it is among such distressed families that fever spreads most rapidly and extensively."<sup>4</sup>

Dr. Cowan attributes the increase of fever in Glasgow, which has steadily been going on since 1816, principally "to the total want of cleanliness among the lower orders of the community, to the absence of ventilation in the more densely peopled districts, and

<sup>1</sup> Tweedie's *Clinical Illustrations of Fever*, p. 78.

<sup>2</sup> *Ib.* p. 80.

<sup>3</sup> *Armstrong on Typhus Fever*, p. 8.

<sup>4</sup> *Edinburgh Medical and Surgical Journal*, vol. xxviii. p. 236.

to the accumulation for weeks or months together of filth of every description in our public and private doghills; to the overcrowded state of the lodging-houses resorted to by the lowest classes; and to many other circumstances unnecessary to mention."<sup>1</sup> The same author, in another part of his statistics, illustrates the causes which tend to render fever epidemic; and he makes the following observations: "From the close of 1836, one of those periodical depressions in trade, arising from the state of our monetary system, has visited this city, and deprived a large proportion of the population of the means of subsistence. From the existence of secret combinations among the working classes in various departments of trade, but especially among the cotton spinners, and the 'strikes' which resulted from these combinations, a very large proportion of the inhabitants, in addition to those already suffering from the state of the money market, were suddenly deprived of employment, and consequently of the means of procuring food. The high price of coal was the means of diminishing the hours of labour, and consequently the amount of wages, in numerous factories, and placed fuel beyond the reach of the lower classes for domestic purposes. And in addition to these sources of misery, the average prices of grain were much higher during 1837 than they had been for some years previously."<sup>2</sup> . . . A reference to the tables of the state of the weather given in the preceding part of this paper, will show the quantity of rain which fell, monthly, during the period of my attendance on the Fever Hospital, and the average temperature indicated by Fahrenheit's thermometer. From these it appears that the quantity of rain was much above the average, while the temperature of almost every month was lower than that of the previous year; and while the mean heat of Glasgow is 47° 75, the mean heat of 1835 was 46° 58, and that of 1836 only 44° 52.<sup>3</sup>

|   |                  |       |
|---|------------------|-------|
| " The number of fever patients treated in Hospital in 1837, was   | . . . . .        | 5387  |
| "                                "                                "                                "        | 1836 " . . . . . | 3125  |
|   |                  | —     |
| Being an increase in 1837 of . . . . .  |                  | 2262  |
| The number of fever patients treated by the district surgeons in<br>their own houses in 1837, was . . . . . |                  | 2320  |
| Ditto             1836, was . . . . .   |                  | 716   |
|   |                  | —     |
| Being an increase of . . . . .  |                  | 1604" |

The above table gives a very inadequate idea of the comparative frequency of fever in 1836 and 1837. During 1836, till the month of December, every applicant for admission was received into the hospital: while in 1837, seldom a day passed without numerous applicants being refused admission for want of room; and many were deterred from applying for admission from a knowledge of the over-crowded state of the wards.<sup>4</sup> Dr. Cowan calculates the num-

<sup>1</sup> Cowan's Vital Statistics of Glasgow. p. 13.

<sup>9</sup> Ibid. p. 33.

<sup>3</sup> *Ib.* p. 16.

<sup>4</sup> *Ib.* p. 37.



ber of fever cases in Glasgow, during the years referred to, as follows :

|          |   |   |   |   |   |                      |
|----------|---|---|---|---|---|----------------------|
| "In 1835 | . | . | . | . | . | 6.180                |
| 1836     | . | . | . | . | . | 10.092               |
| 1837     | . | . | . | . | . | 21.800               |
|          |   |   |   |   |   | <hr/>                |
|          |   |   |   |   |   | 39.072" <sup>1</sup> |

After having given a table of the deaths from fever during each month of the years 1836 and 1837, the same author makes the following deductions from it. "Many interesting observations may be drawn from this table. It shows the slow progress of an epidemic disease when trade is prosperous, compared with what occurs in seasons of distress. Up to November, 1836, the period at which the commercial embarrassments were felt, the mortality from fever had not been rapidly increasing. In November it was just about double what it had been in January preceding, the number of deaths being forty-five in January, and eighty-nine in November. The moment, however, the effects of the stagnation in trade extended to the working classes, the mortality increased with fearful rapidity, aided, no doubt, by the season of the year, the high price of grain, and the scarcity or high price of fuel. . . . The table also marks the period at which the epidemic reached its maximum amount of mortality, namely, in the second quarter of 1837, and in the month of May in that quarter, being the month succeeding that in which the strike of the cotton spinners took place, by which 8000 individuals were thrown out of employment."<sup>2</sup> The total quantity of rain, according to Dr. Cowan, which fell during 1837, as ascertained at the University of Glasgow, was 26.629 inches; and the mean temperature for that year was 46° 31.<sup>3</sup>

We have constructed the following tables from the registers of the weather and the table of deaths from fever in Glasgow, as given in Dr. Cowan's work, in order to show, that though the increased quantity of rain during the years 1836 and 1837 was influential in the diffusion of fever, yet that it had less effect in spreading the epidemic than other causes; for during the year 1837, at which time it had reached its maximum, the total quantity of rain that fell was less than during 1836, while the average temperature of these two years did not materially differ from each other.

<sup>1</sup> Cowan's Vital Statistics of Glasgow, p. 38.

<sup>2</sup> Ib. p. 39.

<sup>3</sup> Ib. p. 35.

| 1836.           | Deaths from fever        | Mean temperature         | Quantity of Rain.          |
|-----------------|--------------------------|--------------------------|----------------------------|
| January . . .   | 45                       | 37° 84                   | 3.868                      |
| February . . .  | 27                       | 35 39                    | 0.732                      |
| March . . .     | 57                       | 38 60                    | 2.375                      |
| April . . .     | 64                       | 42 05                    | 1.098                      |
| May . . .       | 67                       | 51 04                    | 0.173                      |
| June . . .      | 71                       | 55 19                    | 1.812                      |
| July . . .      | 61                       | 54 38                    | 4.536                      |
| August . . .    | 82                       | 53 17                    | 5.317                      |
| September . . . | 56                       | 47 37                    | 2.134                      |
| October . . .   | 89                       | 42 89                    | 4.988                      |
| November . . .  | 89                       | 38 11                    | 2.004                      |
| December . . .  | 133                      | 38 27                    | 2.673                      |
|                 | <sup>1</sup> 841 Total.  | <sup>2</sup> 44.52 Aver. | <sup>3</sup> 31.710 Total. |
| 1837.           |                          |                          |                            |
| January . . .   | 201                      | 36° 13                   | 1.956                      |
| February . . .  | 138                      | 40 55                    | 2.674                      |
| March . . .     | 224                      | 34 39                    | 1.500                      |
| April . . .     | 202                      | 38 28                    | 1.646                      |
| May . . .       | 233                      | 48 21                    | 1.857                      |
| June . . .      | 199                      | 56 89                    | 2.241                      |
| July . . .      | 194                      | 60 80                    | 3.322                      |
| August . . .    | 172                      | 56 90                    | 2.610                      |
| September . . . | 126                      | 52 99                    | 1.570                      |
| October . . .   | 149                      | 48 92                    | 2.997                      |
| November . . .  | 147                      | 39 68                    | 2.293                      |
| December . . .  | 195                      | 41 95                    | 1.963                      |
|                 | <sup>4</sup> 2180 Total. | <sup>5</sup> 46.31 Aver. | <sup>6</sup> 26.629 Total. |

The number of deaths from fever in a given period is not an exact criterion of the number of persons affected ; for the intensity of this disease varies considerably at different seasons of the year ; but as the number of deaths which occurred in each month of 1836 and 1837 respectively, are given, the relative mortality will give a pretty near approximation to the relative number affected.

Dr. Arthur Thomson, in his *Statistics of Fever*, has given two tables, which show the influence of the seasons on the prevalence of fever ; and as his conclusions are drawn from a large number of cases, they are well adapted for illustrating this part of the subject.

<sup>1</sup> Cowan's *Statistics of Glasgow*, p. 38.

<sup>3</sup> *Ib.* p. 4.

<sup>4</sup> *Ib.* p. 38.

<sup>2</sup> *Ib.* p. 5.

<sup>6</sup> *Ib.* p. 35.

"TABLE XII. Showing the maximum, minimum, and mean temperature in Great Britain during each month, from the observations of about thirty years together with the average monthly quantity of rain in inches from thirty-four years' observations (from 1797 to 1830.)<sup>1</sup>

| Months.             | Mean temperature. | Average quantity of rain in inches. |
|---------------------|-------------------|-------------------------------------|
| January . . . . .   | 36°               | 1.90                                |
| February . . . . .  | 38                | 1.49                                |
| March . . . . .     | 43 9              | 1.39                                |
| April . . . . .     | 49 9              | 1.84                                |
| May . . . . .       | 54                | 2.00                                |
| June . . . . .      | 58 7              | 1.94                                |
| July . . . . .      | 61                | 2.55                                |
| August . . . . .    | 61                | 2.15                                |
| September . . . . . | 57                | 2.29                                |
| October . . . . .   | 48                | 2.41                                |
| November . . . . .  | 42                | 2.79                                |
| December . . . . .  | 39                | 2.58 <sup>1/2</sup>                 |

From the above it appears that July and August are the months during which the average temperature is greatest, and that the quantity of rain falling during the last six months of the year is considerably more abundant than during the first six. The following table this author has "compiled from materials selected indiscriminately from all the reports which he could obtain, showing the number of fever cases admitted into the various hospitals in Great Britain and Ireland; but he is chiefly indebted to Drs. Barker and Cheyne's account of the epidemic fever which prevailed in Ireland in 1817-18-19."

"TABLE XIII. Showing that of 51,944 cases of fever admitted into different hospitals in Great Britain and Ireland, the number and relative ratio of admissions in each month were as follow:

| Months.           | No. of Cases admitted. | Relative ratio of admissions per cent. |
|-------------------|------------------------|--|
| January . . . .   | 2895                   | 5.6                                    |
| February . . . .  | 2825                   | 5.4                                    |
| March . . . . .   | 3152                   | 6.1                                    |
| April . . . . .   | 3374                   | 6.5                                    |
| May . . . . .     | 3990                   | 7.6                                    |
| June . . . . .    | 4365                   | 8.3                                    |
| July . . . . .    | 4999                   | 9.6                                    |
| August . . . . .  | 5261                   | 10.1                                   |
| September . . . . | 5046                   | 9.7                                    |
| October . . . . . | 5624                   | 10.8                                   |
| November . . . .  | 5054                   | 9.7                                    |
| December . . . .  | 5359                   | 10.6                                   |
| Total 51,944      |                        | 100.0                                  |

<sup>1</sup> The maximum and minimum temperature is omitted.

<sup>2</sup> Howard on Climate of London, 2d Edit. Vol. i. p. 136.



"It appears from this table that the greatest number of fever cases were admitted into the different hospitals, during the last six months of the year, or from July to December. And the number of cases admitted from January to June are few, compared with the admissions from July to December."<sup>1</sup>

In order to compare the number of admissions in each month, with its mean temperature and average quantity of rain, we shall construct a table out of the two that have just now been quoted; which will show the number of admissions, the mean temperature, and the average quantity of rain for each month.

| Months.        | No. of Cases admitted. | Mean Temperature. | Average quantity of rain in inches. |
|----------------|------------------------|-------------------|-------------------------------------|
| January . . .  | 2895                   | 36°               | 1·90                                |
| February . . . | 2825                   | 38                | 1·49                                |
| March . . .    | 3152                   | 43° 9             | 1·39                                |
| April . . .    | 3374                   | 49° 9             | 1·84                                |
| May . . .      | 3990                   | 54                | 2·00                                |
| June . . .     | 4365                   | 58° 7             | 1·94                                |
| July . . .     | 4999                   | 61                | 2·55                                |
| August . . .   | 5261                   | 61                | 2·15                                |
| September . .  | 5046                   | 57                | 2° 29                               |
| October . . .  | 5624                   | 48                | 2·41                                |
| November . .   | 5054                   | 42                | 2·79                                |
| December . .   | 5359                   | 39                | 2·58                                |
|                | 51,944                 |                   |                                     |

This table shows that the greatest number of fever cases were admitted into the various hospitals from July to December, or during the last six months of the year; and that during this period the average quantity of rain which falls is much greater than during the first six months of the year. If we compare any one month of the last six with any one month of the first, there will be found a similar difference. The same table also shows that the temperature may vary considerably during a similar prevalence of fever, and that nearly the same temperature may prevail with a great variation in the number of cases. Thus, in August the number of cases is 5261, and in December the number of cases is 5359, being a difference only of 98; but the mean temperature of the first-mentioned month is 61°; while that of December is only 39°; the quantity of rain, however, in both of these months is above the average. In March the mean temperature is 43° 9, and the number of cases 3152; while in November the mean temperature is 42°, and the number of cases 5054; but the quantity of rain in March is 1° 39, while in November it is 2° 79, being double the amount of that which falls in the first-mentioned month. In February the mean temperature is 38°, and the number of cases 2825; while in

<sup>1</sup> Edinburgh Medical and Surgical Journal. July, 1838. p. 100.

December the mean temperature is  $39^{\circ}$ , and the number of cases 5359; but the quantity of rain in the first of these months is 1.49 inches, while in December it is 2.58 inches.

The conclusions which may be drawn from this table, are, that in all the months in which the quantity of rain is above the average, fever prevails to a greater extent than in those months in which it is below this point. It does not appear, however, from it that the average range of temperature of this climate has much influence on the prevalence of fever; for if moisture be present, it may prevail to about the same extent, when the average temperature is  $61^{\circ}$ , as in August, or when it is  $39^{\circ}$ , as in December.

The diffusion of fever is thus generally connected with humidity of the atmosphere; yet certainly there are other causes of a more influential kind that are also in operation. This is well exemplified by the two tables (page 52), which show the prevalence of fever and the corresponding weather in Glasgow during the years 1836 and 1837. Thus, although in both of these years the quantity of rain was greater than the average, and in the first of them greater than in the second, yet the number affected with fever during 1836 amounted only to about the half of those that were seized during 1837. The increased prevalence of the epidemic during 1837, must, to a very considerable extent, have depended upon the scarcity of provisions, want of fuel, &c., and their concomitants, filth, &c., which followed the commercial embarrassments of that year.

*An accumulation of persons not previously affected tends to diffuse typhus.* There is also another important point connected with the history of eruptive typhus, which has seldom been taken into calculation in attempting to account for its diffusion, namely, that it does not often attack the same person more than once during his life. Now if this be admitted,—and we have endeavoured to show at page 19, the analogy between typhus and other exanthematous fevers in this particular, but even though M. Hildenbrand's modified view only be granted, namely, that it secures the person who has been affected only for some years—it follows:

1st. That after an epidemic fever has prevailed for some time, it must cease after the lapse of a particular period, from deficiency of *material* to act upon; and the history of almost every pestilence of this kind, shows that it rarely exceeds two years in duration, even in a large city.

2d. That, though fever may constantly exist in a large town, in a minimum proportion, varying in numbers according to the habits of the people, those who have never laboured under the disease are gradually accumulating; and that when the state of the atmosphere, as to humidity, and the scarcity of provisions, with their consequents, filth and deficient ventilation, are concurrent with this accumulation of susceptible individuals, that fever has rarely failed to spread among the community. And if the population of any large city be increasing very rapidly, such as that of Glasgow,

at the rate of ten thousand persons annually, the number of susceptible individuals will be accumulated in a few years to an amount, sufficient for the existence of an extensive epidemic.

3d. That a severe epidemic fever of one or two years' duration is never succeeded by another until several years have elapsed.

A very important inquiry may be deduced from the foregoing statements. Can the very rapid increase of the population of Glasgow account, to a greater or less extent, for its being visited for a series of past years with more frequently recurring epidemics of fever than any other city of Great Britain, similar in size and population? Can the influx of several thousands of unprotected individuals from the country every year afford any explanation of the occurrence; just as there is always a great mortality from smallpox in Glasgow, from the influx of unvaccinated Highlanders, while in many other cities of the kingdom this disease is comparatively rare? There can be no doubt that the influx of so many strangers to this city must have a powerful effect in increasing the number of fever patients; but certainly the filthy and irregular habits of its working population are equally operative as predisponents to contagion. Is there then any prophylactic measure which may either ward off or diminish the extent of an epidemic diffusion of fever in a large city; or is this beyond the control of human means and calculations? We think not; although we do not entertain the notion that fever will ever be completely extinguished in any large manufacturing town; or that the spread of it, epidemically, can be checked *in limine*, when the concurrent circumstances are favourable for its propagation; but certainly much might be done to lessen the intensity of the evil. It has already been shown that filth and deficient ventilation tend much to spread the contagion of typhus, being almost constant concomitants; and that while it generally affects the whole members, or the large proportion of a family, among the lower orders, it rarely spreads in this manner among the better classes of society, who attend more to cleanliness and ventilation. It is quite obvious that an amelioration of the physical condition of the lower orders, in these particulars, would, in proportion as this was effected, diminish their chances of catching the contagion; which would not only operate in lessening directly its diffusion, but, by reducing the number of its sources, must tend to lessen the actual quantity of this principle that might be generated in a given time.

But can this amelioration be effected to any appreciable extent; or if effected, could it be maintained for any length of time? We fear that little permanent amelioration could be effected without a legislative enactment; for though our philanthropists are very active in their charities during the prevalence of an epidemic, it no sooner subsides than they relapse into a comparative quiescence, and our working population into their former habits of filth and intemperance. And the evil will continue to assail us so long as our cities contain so many narrow and filthy lanes, so long as the



houses situated there are little better than dens or hovels, so long as dunghills and other nuisances are allowed to accumulate in their vicinity, so long as these hovels are crowded with inmates, and so long as there is so much poverty and destitution. Why, then, should we not have a legislative enactment that would level these hovels to the ground, that would regulate the width of every street, that would regulate the ventilation of every dwelling-house, that would prevent the lodging-houses of the poor from being crowded with human beings, and that would provide for their destitution? It may be said, that this would interfere too much with the liberty of the subject, and no doubt it would be vehemently opposed by many interested persons. In place, however, of being an infringement on the liberty of the subject, it might rather be designated an attempt to prevent the improper liberties of the subject; for what right, moral or constitutional, has any man to form streets, construct houses, and crowd them with human beings, so as to deteriorate health and shorten life because he finds it profitable to do so? As well ought the law to tolerate the sale of unwholesome food, because it might be profitable to the retailer of it.

---

### CHAPTER III.

Circumstances which tend to render fevers communicable from one person to another.

It is quite obvious, if the doctrine of contagion be admitted, that all those circumstances which favour the diffusion of fevers tend also to render them communicable from one person to another; it is therefore necessary to include them in the following arrangement, although it seems only necessary to illustrate one of them a little farther, namely, the influence of filth and deficient ventilation.

Circumstances which tend to render fevers communicable:

1. Humidity of the atmosphere.
2. Scarcity of provisions, &c.
3. No previous affection with typhus.
4. Filth and deficient ventilation.
5. Age.
6. Acclimatization.
7. Idiosyncrasy of constitution.

Alleged circumstances which tend to render fevers communicable:

1. Weakness of constitution.
2. Greater susceptibility of females.
3. Depressing passions.
4. Intemperance.

Alleged exemptions from fever:

1. From trade or occupation.
2. From chronic diseases.

These different points shall be considered in the following part of the essay, though not exactly in the order enumerated, as this might derange the general connection of the observations.

*Influence of filth and deficient ventilation.* In a previous part of the essay we entered into the consideration of filth and deficient ventilation as tending very powerfully to spread the contagion of typhus; and showed that, where it was concentrated, as in crowded hospitals, or in the small and ill-ventilated houses of the lower classes, it rarely failed to be communicated to the unprotected attendants or inmates of a family. Filthiness of personal habits, however, although it tends to render it more communicable, as we shall endeavour to show from the statistics of the Glasgow Fever Hospital, does not seem to act so powerfully in this respect as deficient ventilation, which by concentrating the contagion may render its operation on the system more certain. In proof of which we may quote the various attendants of our fever hospitals, who are generally very attentive to cleanliness in their persons, and yet, if unprotected, are almost uniformly affected with fever during some period of their attendance, if the wards be in a crowded state. This fact, and the more frequent exemption of the attendants when the wards are moderately filled and well ventilated, seem to prove that contact with the patient is not so essential for the communication of the disease, as being surrounded by an atmosphere highly impregnated with the contagious miasmata. And there are many instances where students have been affected with fever after visiting the wards of an hospital, without having come into contact with the patients or their bed-clothes. There can be no doubt, however, that simple contact of a typhus patient, or of clothes that have been attached to him in any shape, may communicate the disease without the aid of even a partial impregnation of the atmosphere with contagious effluvia, and where the most perfect ventilation has been maintained.

We are at present unacquainted with the channel by which the contagion of typhus most generally enters the body; and though the opinion be generally entertained that the lungs are the organs through which it passes into the system, yet it is equally probable and consistent with analogy and facts to believe, that the skin is, at least, as important as a medium of communication. There are many animal poisons that operate on the system through the skin very powerfully, and yet have little effect when applied to the mucous membrane of the stomach or intestines; now though, in the one case, the poison operated with be ponderable and be applied to the mucous surface of the stomach and bowels, while, in the other case, the contagion of typhus is imponderable, and is applied to the mucous surface of the bronchial tubes, yet, in the absence of direct experiment, this analogy is entitled to some consideration.

The following tables will tend to show that filthiness in personal habits is very frequently connected with the production of typhus, and it includes all the cases of fever that were admitted into the

Glasgow Fever Hospital from May 1st to November 1st, 1839, in whom their state as to cleanliness or filthiness was ascertained:<sup>1</sup>

|   | FILTHY. |          | CLEAN.                   |          |
|---|---------|----------|--------------------------|----------|
|   | Males.  | Females. | Males.                   | Females. |
| Scotch . . . . .                          | 92      | 81       | 64                       | 93       |
| Irish . . . . .                           | 88      | 70       | 60                       | 47       |
| English . . . . .                         | 4       | 2        | 3                        | 3        |
| West Indies and North America } . . . . } | 3       |          |                          | 1        |
|   | 187     | 153      | 127                      | 144      |
| Total filthy...340 cases.                 |         |          | Total clean...271 cases. |          |

The following table shows the number of cases that were filthy and those that were clean in typhus characterised by the eruption, and also the proportions, regarding this point, which were ascertained in febricula;

|  | FILTHY. |          | CLEAN.                                  |          |
|--|---------|----------|---|----------|
|  | Males.  | Females. | Males.                                  | Females. |
| Eruptive typhus                              | 133     | 112      | 73                                      | 77       |
| Febricula . . .                              | 6       | 8        | 19                                      | 15       |
| Total no. of cases of febricula filthy....14 |         |          | Total no. cases of febricula, clean..34 |          |
| of typhus — ...245                           |         |          | of typhus — ..150                       |          |

These two tables show that among 611 cases admitted as continued fever, there were 340 filthy and 271 clean, or about fifty-five per cent. filthy; that among 395 cases of eruptive typhus, there were 245 filthy and 150 clean, or about sixty-two per cent. filthy; and that among forty-eight cases of febricula, there were fourteen filthy and thirty-four clean, or about twenty-nine per cent. filthy. The following deductions may be drawn from these facts. 1. That the proportion of filthy persons is greater than that of the clean among the whole number of cases admitted, and including not only typhus, but bronchitis, febricula, pneumonia, and several other affections which are specified in a table already given. 2. That among the eruptive or decided cases of typhus, the proportion of the filthy to the clean is still greater than what exists among the whole number of cases. 3. That among the cases of febricula, the proportion of filthy persons is only twenty-nine per cent., while in eruptive typhus it is sixty-two per cent. 4. That, as the proportion of filthy persons in the whole number of cases is less than in those affected with eruptive typhus, it is fair to infer that this is owing to an admixture, with the latter, of febriculous, bronchitic cases, &c., since it has been shown that filthiness is much less fre-

<sup>1</sup> The reports, respecting the clean or filthy state of the patients admitted, were taken by the barber of the Fever Hospital, and afterwards transferred by the author, along with other statistical facts, into his own journal.



quently a concomitant of febricula, &c., than it is of eruptive typhus.

We are entirely ignorant of the nature of those substances which absorb the typhus contagion with most facility; but, as filth is very frequently a concomitant of its ready communicability, it may be assumed that either the clothes or the deposits on the skin of filthy persons have a tendency to absorb the contagion and to retain it until the system become affected. We know that certain gases, and even odours and fetid effluvia, are absorbed more readily by some substances than by others; and though we are only warranted to assume from this analogy, that typhus contagion is very probably regulated by a similar law, yet, on the other hand, if want of cleanliness facilitate the operation of contagion on the system, it is not possible to explain this effect on the principle of pulmonary inhalation, while the theory of cutaneous absorption is not opposed by any fact or analogy. If this view be adopted, it will obviously lead to a prophylactic measure of considerable importance, namely, the daily and thorough ablution of the skin, and the frequent changing of the wearing apparel; for it is not probable that the contagion will be absorbed immediately after its application to the clothes or skin of the person who has been exposed to it; and by the daily ablution of the whole body, it may be removed before this can occur.

*Influence of idiosyncrasy of constitution.*—The contagion of typhus is not communicable to all persons with the same facility. Some individuals are infected after the first exposure, while others may be exposed for weeks, or even many months, almost constantly, before they are attacked. It may be said, however, that in this last case the contagion has remained for a longer time latent than in the former. There is no very precise evidence existing as to this point, and the opinions entertained by many authors are often conjectural. Dr. Bancroft states as follows: "It results, therefore, from this statement, that among the ninety-nine orderlies and nurses who had probably not been exposed to the contagion before their attendance on the sick commenced, the earliest attack was on the thirteenth day, and the latest on the sixty-eighth; but these returns were made up about the 20th of April, and it appears that some who had escaped till that time were afterwards attacked; and therefore, though there may be reason to conclude that febrile contagion does not remain inactive so long after being received into the body as marsh miasmata, I see none for believing that an interval of five or six months may not sometimes elapse before the actual production of fever by it."<sup>1</sup> Dr. Perry is of opinion, that "the earliest period of the disease making its appearance after exposure to contagion is eight days, more frequently fourteen, and sometimes so long as two months."<sup>2</sup> What the circumstances are which render some persons, who enjoy good health, are well fed

<sup>1</sup> Bancroft on Yellow Fever, &c. p. 516.

<sup>2</sup> Edinb. Med. and Surgical Journal, vol. xlv. p. 69.

and cleanly in their personal habits, more susceptible of contagion at one period than at another are totally unknown; but we are in the same state of ignorance as to the reason why scarlet fever, &c., may be caught at one period and not at another, and why vaccination frequently succeeds at last after five or six unsuccessful trials. Again, a certain proportion of persons appear not to be susceptible of the disease. Dr. Perry is the only author that we are acquainted with that enumerates the proportions of susceptible and non-susceptible individuals. His tenth proposition is the following: "That between the ages of seven and fifty, sixteen out of twenty are susceptible of being affected with contagious typhus, if exposed to the contagion, and not protected by having previously had the disease."<sup>1</sup>

That there is a certain proportion of individuals who are not susceptible of typhus contagion there can be no doubt, for there are many medical practitioners and nurses of fever-hospitals who have never laboured under the disease, although they have been exposed to its influence for many years, but there is no proof that in the present state of our fever statistics we can define the proportion of unsusceptible persons. It is a common opinion that constant exposure lessens the susceptibility to fever; but this, in the present state of our knowledge, can only be considered as a probable hypothesis.

*Influence of sex.*—Hildenbrand is of opinion that delicate men, who have a fine skin and feeble bodies, are most subject to contagion; while, on the contrary, those that are robust, plethoric, vigorous, and well nourished, more seldom contract it. These opinions are entertained by several writers on fever; and for a similar reason, it is sometimes concluded that females are more subject to this disease than males. It is natural for an author, who advocates the absorption of contagion by the skin, such as Hildenbrand, to infer that a fine skin, like that of the female, will absorb more readily than one which is coarse; and although this theory be supported by the statistics of some hospitals, it is opposed by those of others. The number of admissions into the Glasgow Fever Hospital during the year 1836 were 1116 males and 1141 females,<sup>2</sup> which is only a small excess of females; but if the excess of the female over the male population of Glasgow be taken into the account as about one sixth, the proportion of males that have been affected with fever will be plus instead of minus. In the same institution were admitted, from May 1st to November 1st, 1839, 270 males and 276 females, classified under typhus. Into the Cork-street Fever Hospital, Dublin, from 5th January, 1817, to 30th April, 1818, there were admitted 2883 males and 2849 females, which is a small excess of males.<sup>3</sup> Again, in other hospitals, there has occurred an excess of females. There were admitted into the Waterford Hos-

<sup>1</sup> Edinb. Med. and Surgical Journal, vol. xlv. p. 67.

<sup>2</sup> Cowan's Vital Statistics, p. 19.

<sup>3</sup> Barker and Cheyne on Fever, vol. i. p. 91.

pital 1277 males and 1452 females,<sup>1</sup> into the London Fever Hospital 1229 males and 1308 females,<sup>2</sup> into the Limerick Fever Hospital 1332 males and 1895 females, being a large excess of females,<sup>3</sup> and into the Edinburgh Royal Infirmary 962 males and 1075 females.<sup>4</sup> The facts which have been hitherto published regarding the susceptibility of the different sexes to fever are not yet sufficiently extended to warrant us drawing any certain conclusion from them; but certainly it does not appear to be established by satisfactory evidence, that the one sex is more liable to the disease than the other; and, where this does occur in any particular place, that it cannot be accounted for by the general excess of female population in large cities, or by other circumstances connected with their history. Drs. Barker and Cheyne remark, that "in Dublin, when the epidemic had completely established itself, the males admitted to hospital were most numerous, but in its progress the admissions of females exceeded those of males. . . . As to the comparative frequency of fever in the male and female sex in the country at large, we can form no decisive opinion, the answers to our inquiries on that head not having been perfectly satisfactory."<sup>5</sup>

Although the comparative frequency of fever among the sexes has not been accurately determined, it has been proved satisfactorily by the statistics of almost every large hospital, that a larger per cent. of males than of female patients die of the disease; and it is proved by the Glasgow Mortality Bills, that a much greater number of the male than of the female population of that city are carried off by it. Thus, in Glasgow, during the year 1836, 465 males and 376 females died of fever; during 1837, 1187 males and 993 females; and during 1838, 439 males and 377 females.<sup>6</sup> If the average mortality of each sex could be accurately ascertained, this large amount of deaths might be made available for determining the liability of the different sexes to fever, by the same method of approximation which Dr. Cowan has adopted in calculating the amount of fever in Glasgow during the years 1836 and 1837;<sup>7</sup> but as the proportionate mortality of the different sexes is not the same during every season, and as it may not be the same among those treated at home as in those treated in hospitals, this method, although well adapted for giving a general approximation, is not well calculated for determining a nice question of this kind.

*Influence of delicacy, or weakness of constitution.*—We have already remarked, that it is a prevalent opinion among medical

<sup>1</sup> Barker and Cheyne on Fever, vol. i. p. 193.

<sup>2</sup> Dr. S. Smith's Treatise on Fever, p. 432.

<sup>3</sup> Dr. Geary's Report, Dublin Journ. of Med. Science, vol. xii. p. 10.

<sup>4</sup> Edinb. Med. and Surg. Journal, Oct. 1839, p. 448.

<sup>5</sup> Barker and Cheyne on Fever, pp. 89-90.

<sup>6</sup> Glasgow Bills of Mortality for 1836, 1837, and 1838.

<sup>7</sup> Dr. Cowan estimated the proportion of the whole mortality in 1837 as one in every ten patients; and to determine the amount of cases, multiplied the whole number of deaths, which were 2180 by 10 = 21,800.



men, that persons naturally weak and delicate are more liable to fever than those who are healthy and vigorous. This opinion seems to be as little capable of proof as the preceding one regarding the greater liability of females to the disease. We are not, however, in possession of much evidence, and none statistical, so far as we are aware, regarding this point, beyond the loose and general observations of authors.

We have kept a record of the physical habit of the patients admitted into the Glasgow Fever Hospital from May 1st, to November 1st, 1839, and the following were the divisions adopted :

1. Moderate, by which is meant a person having an ordinary quantity of muscle and cellular substance.
2. Full or plethoric, having an extra quantity of adipose texture or of blood.
3. Muscular.
4. Spare.
5. Emaciated or unhealthy in appearance.

|                           | Males. | Females. | Total,    |
|---------------------------|--------|----------|-----------|
| Moderate . . . . .        | 116    | 93       | 209       |
| Full or plethoric . . . . | 28     | 73       | 101       |
| Muscular . . . . .        | 44     | —        | 44        |
| Spare . . . . .           | 24     | 41       | 65        |
| Unhealthy or emaciated .  | 2      | 8        | 10        |
|                           |        |          | <hr/> 429 |

The whole of these 429 cases were characterised by the typhoid eruption, and will therefore be considered as decided cases of typhus. It appears from this table, that there were only ten cases in an emaciated or unhealthy condition ; and almost all of them, as far as could be ascertained, were engaged in their ordinary occupations at the time of their seizure. The spare and unhealthy, when added together, only form about 17 per cent. of the whole number.

*Influence of chronic diseases.*—The evidence, such as we have collected from the previous history of patients admitted into the Glasgow Fever Hospital, and from post mortem examinations, seems to prove that persons affected with any particular chronic disease of the chest or belly, are very rarely affected with typhus fever. Hildenbrand states that phthisical persons are very rarely affected with typhus fever ; and that, out of many hundred cases of this disease that he has treated, not one instance of a phthisical person has occurred. We have heard the same opinion expressed by several physicians of extensive hospital experience, and that they have scarcely ever met with a case of tubercles in a person who has died of eruptive typhus.

This opinion we can nearly confirm from our own experience,

for out of more than 100 post mortem inspections we have met with only three cases ; and the number of tubercles in each did not exceed three, which were small and only partially softened.

*Influence of fear and the depressing passions.*—The influence of fear and the depressing passions has also been considered as very powerful in predisposing persons to be affected with typhus contagion. There can be no doubt that fear has a tendency to produce a temporary depression of the physical powers ; but, as has been already shown, there is no proof that persons of a naturally spare or weak habit of body, who are generally very sensitive, are more liable to fever than those of an ordinary constitution, this opinion must also be considered hypothetical. Indeed, the facts, as far as our inquiries have enabled us to judge, seem to prove that the apprehension of fever, more particularly when it is not epidemic, is very rarely felt until the person is actually seized with the disease ; for some cannot recollect of a single circumstance by which they could be exposed to contagion ; and a considerable number of those who had undoubtedly been exposed to it, were only made aware of the fact when it had been elicited by cross examination. We are quite aware that cases may be brought forward, of sensitive individuals who have been seized with fever soon after visiting a person labouring under the disease ; but as this fact can be opposed with at least an equal number of persons who were destitute of fear, and yet caught it after an exposure to contagion, no conclusion whatever can be drawn from them. It must be observed, however, that though there is no proof that persons who are naturally weak in body or of a sensitive disposition are more susceptible of fever than those who are naturally vigorous and robust, yet that, during famine or commercial distress, poverty by depressing the mind and lowering the physical status from insufficient aliment, does powerfully predispose a community to become affected with fever. This has been already shown in a former part of the essay ; and has been again alluded to, in order that the distinction might be made between an individual of naturally weak mental and physical stamina, and one who has been reduced to that state by deficient nutriment.

*Influence of intemperance.*—It is a question of vital importance to the inhabitants of large towns, whether intemperance predisposes those who indulge in it to be affected with fever. A solution of this point in a satisfactory manner cannot, we are afraid, be made from our present data ; for no statistics regarding it have been published. Indeed, it is sometimes very difficult, even after the most careful inquiries, to find out the habits of patients who are sent to an hospital ; for most of them are ashamed to acknowledge intemperance when it does exist, and those who admit that they indulge a little are sometimes more abstemious, in point of quantity, than those who deny any indulgence whatever. The ascertaining of such habits, accurately, is in many cases impossible, and the evidence must be viewed principally as an approximation to the

truth. At the same time, this approximation may be often rendered very convincing, by sifting the answers of the patients, by an attentive examination of their appearance, and by the evidence of friends; and occasionally conclusions confirmatory of the opinion formed may be drawn from their trade or occupation; for it is well known, that in some occupations the majority of the workmen are addicted to excessive drinking. The frequent combination of drunkenness with filthiness of personal habits is another circumstance which complicates this question very materially, and renders the appreciation of the value of each a matter of some difficulty. The following table shows the proportion of temperate and intemperate individuals, that were admitted into the Glasgow Fever Hospital from November 1st, 1838, to November 1st, 1839, whose habits could be ascertained with more or less certainty; and the eruptive cases are only included.

|                     | Temperate. | A little Intemperate. | Intemperate. |
|---------------------|------------|-----------------------|--------------|
| E. Typhus (MALES)   | 125        | 51                    | 73           |
| E. Typhus (FEMALES) | 76         | 8                     | 30           |

In this table the proportion of intemperate males is much greater than that of the females. Can this circumstance account, to a greater or less extent, for the greater mortality of the former in almost all hospitals? It would be natural for a person, who wished a certain theory supported, to conclude that as such a large number of those affected with fever were reported to be more or less intemperate, this could not be an accidental and uninfluential concatenation; but that the two circumstances must stand to one another, in the relation of cause and effect. It would be necessary, however, before such an inference could be drawn, to ascertain whether the proportionate amount of the intemperate to the sober was greater in the cases of fever than what existed among the community from whom they were sent. We fear that this question cannot be determined; for the prevalence of intemperance among the working population of large cities has been calculated principally from the amount of drunkards that appear on our streets, from the large and increasing number of our spirit shops, and from the enormous quantity of ardent spirits consumed in a year. And though there can be no doubt that drunkenness has increased among the lower classes to a lamentable extent, its numerical amount has never been ascertained, and perhaps never can be accurately ascertained; but certainly there are grounds for believing that the proportion enumerated as intemperate, in the table which has been given, is not greater than what really exists among the inferior grades of our working population. A similar opinion is entertained by Chomel, who states that alcoholic excesses appear to exert no influence on the production of the typhoid fever. Intemperance, however, tends indirectly to predispose the system to contagion, by the production of filthy habits. It also exercises a most powerful influence in increasing the mortality from fever. In the Glasgow Fever Hospital there occurred eighty-one deaths from eruptive



typhus in individuals whose habits were ascertained, and thirty-four of these were reported as intemperate, nineteen a little intemperate, and twenty-eight temperate. In Dr. Craigie's table of the deaths in thirty-one fever cases that occurred in the Edinburgh Royal Infirmary, there are fifteen stated to be irregular or dissipated, only two regular, the habits of the remainder are not stated.<sup>1</sup>

It is also a singular fact, which has been noticed by several writers, that fever is more fatal among the higher than among the lower classes. Dr. Braken states, in reference to the fever which prevailed at Waterford during the years 1817-18-19, that "it would be difficult to adjust the rates of mortality in the upper classes, but it seems probable that one fourth or perhaps one third of all those persons who were attacked with fever fell victims to its power."<sup>2</sup>

Drs. Barker and Cheyne, in their historical account of the Irish epidemic, state that "in every part of the country fever was reported to have been much more fatal amongst the upper than the lower classes."<sup>3</sup> To what is this difference of mortality, so generally remarked by experienced hospital physicians, to be attributed? and which in Ireland seemed to be very remarkable, namely, in the lower classes about one in twenty-three cases, and among the upper classes one in three or four generally, but in other places about one in seven. Can the difference in the mode of living account for this anomaly? as the first live very much on potatoes, while the other use a larger or smaller proportion of animal food; and the lower classes almost every where in this country use less animal food and stimulating dishes than those who are more wealthy and in a higher sphere of society.

This subject is highly worthy of farther investigation; for the difference of mortality which exists among these different classes most probably depends more upon some cause connected with their habits and kind of aliment than upon their *morale*.

*Influence of age.*—Almost all modern authors who have written on fever statistically state that the susceptibility to this disease is greater among young persons than among the old; and there is sufficient evidence brought forward to establish this; but certainly the conclusions which have been drawn respecting the greater liability of one period of youth when compared with another have not been satisfactorily proved.

From an examination of the ages of 117 patients, and by comparing his table with the results obtained by M. Louis and some other observers, M. Chomel thinks it may be established that the most common period of life for attacks of typhoid fever is from the eighteenth to the thirtieth year; that it is rarely observed beyond

<sup>1</sup> Edinburgh Medical and Surgical Journal, vol. xlvii. p. 296.

<sup>2</sup> Barker and Cheyne on Fever, vol. i. p. 277.

<sup>3</sup> Barker and Cheyne on Fever, vol. i. p. 95.

forty years; and that perhaps no case has yet been observed where the patient was beyond fifty-five years.<sup>1</sup>

Dr. Cowan states that, "from an examination of these (his) tables, it appears that the period of life at which fever is most liable to occur is from the age of twenty to twenty-five years for the males, when the proportion is 21.23 per cent., and from the age of fifteen to twenty for females, when the proportion is 23.83 per cent."<sup>2</sup> Dr. Geary, in his report of the Limerick Fever Hospital states, "that children are much more liable to fever than is generally supposed, and to the little apprehensiveness of disease being transmitted by them may be attributed the spread of disease through families in many instances. It will be seen underneath that nearly one sixth of the admissions for 1836 were under ten years of age, a fact which bears out what we have stated, and is also a satisfactory proof of the increasing confidence which public hospitals are acquiring from the community. . . . Of the entire treated for the year, full two thirds were under twenty years of age."<sup>3</sup> We have selected the statistics of these two last mentioned authors chiefly on account of the large number of cases from which their conclusions have been drawn—the first having treated 2257, and the second 3227—in order to show the fallacy of the principles by which the susceptibilities of persons to fever at the various periods of life are estimated. It is obvious that the proportionate number of cases at the various ages given by the above authors is only that which exists in an hospital; but it by no means follows that the same ratio will be maintained among the general community.

Before any such inference could be drawn, evidence must be brought forward to prove that the admissions of cases into hospitals were in the same proportion as to ages as that which existed among the population from whom they were sent; for it is well known that children in many towns are not so frequently sent to hospitals as adults. And this circumstance may perhaps account for the discrepancy which exists between the conclusions of Dr. Cowan and those of Dr. Geary. This method, however, even though it were ascertained that the same proportionate number of cases affected with fever was admitted into hospitals at the various ages, is very unsatisfactory, as has been pointed out by Dr. Arthur Thomson; for it does not show the number of persons living at each period of life, so that an estimate may be formed of the proportion which the number living at each term of life bears to those who have been attacked. In order to supply this deficiency, the author we have already quoted gives the following table:

<sup>1</sup> Chomel, *Clinique Médicale*, vol. i. p. 311.

<sup>2</sup> Cowan's *Vital Statistics of Glasgow*, p. 20.

<sup>3</sup> *Dublin Journal of Medical Science*, vol. xii. pp. 98-9.

"TABLE IV.—Showing the estimated number in the inhabitants at Glasgow at each age during the year 1836; the number attacked by fever, together with the ratio attacked out of every thousand at each decennial period of life.

| Ages.    | No. of inhabitants at each age. | No. attacked by fever. | Ratio per 1000 attacked by fever. |
|----------|---------------------------------|------------------------|-----------------------------------|
| Under 10 | 67.469                          | 3811                   | 56                                |
| 10 to 20 | 50.009                          | 1539                   | 30                                |
| 20 to 30 | 46.275                          | 1611                   | 34                                |
| 30 to 40 | 32.044                          | 911                    | 28                                |
| 40 to 50 | 21.758                          | 392                    | 17                                |
| 50 to 60 | 14.090                          | 294                    | 20                                |

"It appears from this table that the greatest susceptibility to fever occurred under ten years of age, after which fever occurs most frequently among persons between the age of twenty and thirty. The number attacked after the age of thirty decreases gradually as life advances."<sup>1</sup>

This method of calculating the susceptibilities to fever is certainly superior to that which is deduced from the admissions into hospitals; but it is attended with the following objections, which must tend to lessen the accuracy of the conclusions :

1. The number of fever cases stated in the table is not the result of actual observation, but is calculated from the rate of mortality which occurred at the various terms of life in a fever hospital, on the same principle that Dr. Cowan endeavoured to ascertain the amount of fever in Glasgow, and which is explained at page 62; consequently, the deduction is only an approximation to the truth.

2. The diseases of which persons die in Glasgow are reported by their friends and not by their medical attendants; and though we acknowledge the great value and utility of the mortality bills, even upon this imperfect plan, certainly errors respecting diseases which are sometimes difficult to distinguish from others must frequently take place. This is particularly the case with fever in childhood; which is not so easily recognised as small-pox, measles, and scarlet fever, and which is frequently confounded with hydrocephalus, teething, derangements of the chylopoietic viscera, bronchitis, &c.

3. If it be admitted that typhus does not frequently attack individuals more than once in their lives, or even upon the principle of its protecting them only for a certain number of years, it follows that there must be a greater number secured by a previous attack among those at the more advanced periods of life than among those who are young. This point has not been prominently alluded to, so far as we are aware, in any previous account of the disease; but in calculating the susceptibilities of persons to fever, those who

<sup>1</sup> Edinburgh Medical and Surgical Journal, July 1838, p. 92.



have previously undergone the disease, or at least a portion of them, ought to be deducted from the general population.<sup>1</sup> This subject must, therefore, be considered as not thoroughly investigated; and perhaps will remain so until there be some legislative enactment compelling medical practitioners to make a return of all the diseases which have been treated by them throughout the year.

The observations of the British and Irish physicians do not agree with those of M. Chomel, as to the maximum and minimum period of life, beyond which persons are not susceptible of typhus or the typhoid fever. The last mentioned author thinks it very rarely occurs below ten years of age, and that perhaps no case has occurred where the patient was beyond fifty-five years. Into the Glasgow Fever Hospital there were admitted, during the year 1836, 2257 cases of fever; and out of this number there were forty-one under five years of age, and three between seventy and seventy-five years.<sup>2</sup> Into the Limerick Fever Hospital, during the year 1836, there were admitted 3227 cases of fever, and there were eighty-one below five years of age, and ten between sixty-five and seventy years.<sup>3</sup> Dr. Craigie treated in the Edinburgh Royal Infirmary seven cases of fever between sixty and seventy years, among 343 admissions.<sup>4</sup>

We have met with, in the Glasgow Fever Hospital, five cases of eruptive typhus in children reported to be three years of age, from the 1st May to 1st November, 1839.

*Acclimatization.*—M. Chomel and some other French authors state that the typhoid fever attacks most readily those who have been only a short time in Paris, while those who are natives of that city are more frequently exempted. He mentions that among ninety-two individuals, sixty-four, that is to say more than two thirds, had lived in Paris less than two years, while two only were natives and residents. The small number of those who were born and resided in Paris is certainly remarkable; at the same time it must be kept in mind that no patient was admitted into his wards below fifteen years of age.

We have constructed the following table in order to illustrate this part of the subject; and it comprehends 568 eruptive cases, which were admitted into the Glasgow Fever Hospital from November 1st, 1838, to November 1st, 1839. It shows the number of patients born in Glasgow, the number of strangers, and the duration of their residence in Glasgow.

<sup>1</sup> Dr. Cowan calculates that about 38,000 persons were affected with fevers in Glasgow during the years 1835, 1836, 1837.

<sup>2</sup> Cowan's Vital Statistics of Glasgow, p. 20.

<sup>3</sup> Dublin Journal of Medical Science, vol. xii. p. 99.

<sup>4</sup> Edinburgh Med. and Surg. Journal, vol. xlv. p. 35, and vol. xlvii. p. 329.

|                                      | Males.    | Females.  | Total.    |
|--------------------------------------|-----------|-----------|-----------|
| Natives of Glasgow . . . . .         | 77        | 99        | 176       |
| Strangers resident from 1 to 14 days | 12        | 4         | 16        |
| 2 weeks to 1 month . . . . .         | 7         | 6         | 13        |
| 1 to 2 months . . . . .              | 10        | 14        | 24        |
| 2 to 3 months . . . . .              | 10        | 8         | 18        |
| 3 to 4 months . . . . .              | 5         | 5         | 10        |
| 4 to 5 months . . . . .              | 5         | 3         | 8         |
| 5 to 6 months . . . . .              | 9         | 12        | 21        |
| 6 months to 1 year . . . . .         | 29        | 26        | 55        |
| 1 to 2 years . . . . .               | 24        | 17        | 41        |
| 2 to 3 years . . . . .               | 13        | 10        | 23        |
| 3 to 4 years . . . . .               | 6         | 11        | 17        |
| 4 to 5 years . . . . .               | 12        | 4         | 16        |
| 5 to 10 years . . . . .              | 29        | 32        | 61        |
| 10 to 20 years and upwards .         | 36        | 33        | 69        |
|                                      | <hr/> 284 | <hr/> 284 | <hr/> 568 |

It appears from this table that among 568 eruptive cases of typhus, in whom this point was ascertained, 176 were natives of Glasgow, and 392 were strangers: 206 of these strangers had resided in Glasgow only from one day to two years, and 186 from two to twenty years and upwards. The strangers amount to about sixty-nine per cent. of the whole number of cases; and those who were affected within two years of their residence in Glasgow to about fifty-two per cent. of the whole number of strangers.

The following deductions may be drawn from these facts: 1. That strangers are more liable to become infected with typhus fever than native residents. 2. That the majority of strangers are infected within a comparatively short period of their residence in Glasgow. 3. That a minor proportion of the strangers, like the natives of Glasgow, may escape infection for many years, and yet be afterwards attacked. These results support the views which we have elsewhere given of the laws of typhus.

Most of the strangers come from country districts, in which it may be fairly presumed that typhus does not constantly exist, as it does in large towns; it is therefore probable that the majority of them are unprotected by any previous attack; for if typhus attack an individual many times during his life, why should the natives of a town containing 263,000 inhabitants, who are constantly within the sphere of contagion, bear so small a proportion to the strangers.

The facts connected with the propagation of small-pox in Glasgow are of a very similar kind; for the majority of the unvaccinated persons who are sent to the Fever Hospital are Highlanders, who have come very recently from a district where this disease is not in operation, and who consequently have not previously been exposed to contagion.

*Influence of trade or occupation.*—Little is known accurately as to the operation of the different trades, in increasing or diminishing the susceptibility to fever. In manufacturing towns there are a greater number of persons connected with cotton manufactures affected with fever than other operatives; but this may be expected; because they generally in such places constitute the most numerous class among the general population. Again, in other towns, labourers are the most numerous class who are affected with fever. Dr. Geary, Physician to the Limerick Hospital, states that “we have a tabular view before us, which shows the number in families of each class of 2416 persons admitted from the city parishes, and the proportion they bear to each other; though the exact relation to the general population cannot be determined, as there is considerable difficulty in ascertaining the amount of each trade. However, as may be expected, the labouring class being the most numerous, constitute the largest number, averaging one half of the entire; and including all, we find that more than one half of those treated for the year cannot be said to be of any trade, namely, females and children.”<sup>1</sup>

It is an ancient opinion that tallow-chandlers, butchers, tanners, and water-carriers are rarely affected with plague or fever. Dr. Hancock quotes the following evidence in reference to the trades that were exempted from the plague. “Volney tells us that at Cairo it is observed that water-carriers, continually wet with the fresh water they carry in skins upon their backs, are never subject to the plague. This fact coincides with the observations in London. George Baldwin, consul-general in Egypt, says that among upwards of a million of inhabitants carried off by the plague in Upper and Lower Egypt, during four years, he could not learn that a single oilman or dealer in oil had suffered. Jackson, in his reflections on the commerce of the Mediterranean, likewise informs us, that in the kingdom of Tunis, there never was known an instance of any of the coolies or porters who work in the oil stores being in the least affected by the disorder; their bodies being always well smeared with oil, as well as their clothes being imbued with it. We are told by Fonseca, that all the tanners at Rome escaped the plague; and Mindererus and Schenck make a similar observation. Dr. Maclean refers to the exemption of tanners at Cairo.”<sup>2</sup> Dr. Tweedie notices the exemption of butchers from fever, and states that though almost every description of mechanics was admitted during the year into the London Fever Hospital, he did not recollect of a single instance of a butcher.<sup>3</sup> Other physicians, however, have met with patients who followed this occupation. Dr. Southwood Smith, in his table of the occupations of 679 patients affected with fever, enumerates three butchers, two

<sup>1</sup> Dublin Journal of Medical Science, vol. xii. p. 103.

<sup>2</sup> Hancock on Pestilence, p. 184.

<sup>3</sup> Tweedie's Clinical Illustrations of Fever, p. 79.



curriers, and two skinners.<sup>1</sup> Dr. Craigie, in his table of 181 cases of fever treated in the Edinburgh Royal Infirmary, mentions three butchers among that number.<sup>2</sup>

The following tables show the various trades, occupations, &c. of 586 patients admitted into the Glasgow Fever Hospital from November 1st, 1838, to November 1st, 1839. They include all the eruptive cases of typhus in which the occupation, &c. were ascertained.

## MALES.

|                        |                       |                       |
|------------------------|-----------------------|-----------------------|
| Bricklayer . . . 1     | Fisherman . . . 1     | Plasterer . . . 1     |
| Brushmaker . . . 1     | French-polisher . . 1 | Pensioner . . . 1     |
| Brickmakers . . . 2    | Glass-cutters . . . 3 | Printers . . . 2      |
| Blacksmiths . . . 9    | Glass-blowers . . . 3 | Quill-dresser . . . 1 |
| Bakers . . . . 4       | Gasmaker . . . . 1    | Quarriers . . . . 2   |
| Currier . . . . 1      | Gardener . . . . 1    | Ropemaker . . . . 1   |
| Confectioner . . . 1   | Ham-curer . . . . 1   | Schoolmaster . . . 1  |
| Collier . . . . . 1    | Hawkers . . . . . 5   | Lawyer . . . . . 1    |
| Cooper . . . . . 1     | Joiners . . . . . 6   | Showman . . . . . 1   |
| Cabinet-makers . . 3   | Labourers . . . . 76  | Shoemakers . . . 11   |
| Carters . . . . . 4    | Last-maker . . . . 1  | Sailors . . . . . 6   |
| Carpenters . . . . 3   | Malsters . . . . . 3  | Factory-workers . 22  |
| Candle-maker . . . 1   | Masons . . . . . 6    | Servants . . . . . 4  |
| Clerks . . . . . 2     | Milk-dealer . . . . 1 | Slaters . . . . . 3   |
| Coffee-roaster . . . 1 | Optician . . . . . 1  | Tailors . . . . . 7   |
| Dyers . . . . . 3      | Nailers . . . . . 4   | Tinsmith . . . . . 1  |
| Engineers . . . . . 7  | Policeman . . . . . 1 | Turner . . . . . 1    |
| Engineman . . . . 1    | Porters . . . . . 4   | Tobacconist . . . . 1 |
| Firemen . . . . . 3    | Painters . . . . . 3  | Wireworkers . . . . 2 |
| Founders . . . . . 4   | Potters . . . . . 3   | Weavers . . . . . 63  |
|                        | Watchman . . . . . 1  | Warehouseman . . . 1  |
| 53                     | +                     | 126                   |
|                        |                       | +                     |
|                        |                       | 133=312 tot.          |

## FEMALES.

|                      |                         |                         |
|----------------------|-------------------------|-------------------------|
| Weavers . . . . 11   | Servants . . . . 38     | Hawkers . . . . . 6     |
| Factory-workers 77   | Fruit-dealers . . . 2   | Bark-peeler . . . . 1   |
| Sewers . . . . . 25  | Washerwomen . . . 2     | Stocking-knitter . . 1  |
| Beggar . . . . . 1   | Winders of Yarn . . 3   | Straw hat-maker . . . 1 |
| Shearers . . . . . 3 | Calico-printers . . . 2 |                         |
| Married . . . . . 97 | Nurses in F. Hosp. . 4  |                         |
| 214                  | +                       | 51                      |
|                      |                         | +                       |
|                      |                         | 9=274                   |

Total of Males and Females=586

*Influence of Pregnancy.*—Among 172 females admitted from May 1st to November 1st, 1839, there were fourteen pregnant, being about eight per cent. of the whole, and fully three fourths of this number had abortion or premature labour during the course of the disease. This appears a considerable number; but in the present state of our knowledge respecting this point, we are only entitled to conclude from it that pregnancy is not an operative circumstance

<sup>1</sup> Southwood Smith's Treatise on Fever, p. 431.

<sup>2</sup> Edinb. Med. and Surgical Journal, vol. xlvii. p. 286.

in preventing the communication of typhus, and this opinion is corroborated by the *general experience* of practitioners. Unless there existed a correct enumeration of the number of individuals belonging to each occupation in Glasgow, no particular deduction could be drawn from these tables; but certainly it is worthy of remark that there should be no butcher,<sup>1</sup> no tanner, only one currier, only six masons, and one bricklayer, who together are a very numerous class of operatives in Glasgow, while there are seventy-six labourers. The latter class of operatives are generally filthy in their habits and live in small ill-ventilated houses, while masons are comparatively cleanly and comfortable in their circumstances.

The evidence which exists on this point, as has been already stated, is still very imperfect and inconclusive; but certainly butchers and tallow-chandlers or candle-makers appear to be more rarely inmates of a fever hospital than persons belonging to other trades and occupations who are as numerous in the general population. But there are several circumstances which influence the admissions into hospitals, which ought to be taken into consideration before any conclusion can be drawn from them. 1st. Those operatives who are in better circumstances than the average class of them, with the exception of servants, are more rarely sent to an hospital. 2d. There may exist prejudices in a particular class of operatives against hospitals. Whether any of these objections may apply to the butcher or the candle-maker we are unable with certainty to determine, but undoubtedly the persons who followed these two occupations are not below the average in point of comfort in their circumstances.

M. Parent-Duchatelet has made some very curious and important experiments respecting the absorption of putrid emanations by various substances, which may, by analogy, be made to bear upon this subject. He found that distilled water and soups possessed, in a high degree, the property of impregnation with putrid effluvia; but that greasy bodies covering the surface of the liquid oppose an obstacle to the passage of these emanations. The following is his eighteenth experiment: "It might be useful to know if there were any means capable of preventing liquids from being impregnated with putrid emanations; this means chance furnished me with. Having set aside a certain quantity of *bouillon* as an experiment, I found it next day covered with a pellicle of grease, and below this grease it was in a most natural state; inferring from this experiment I poured two or three drops of oil into each of the experimental dishes filled with *bouillon*, as well as into the others filled with water, and after they had remained twenty-four hours among the putrid emanations I remarked that none of these liquids had contracted odour, but the surface of oil gave out in all the cases a

<sup>1</sup> One patient had been a butcher, but had worked as a labourer for six months before he was affected.

very powerful odour."<sup>1</sup> Solid substances were also infected with the odour of putrid emanations, such as beef and wood,<sup>2</sup> and water, completely inclosed in a piece of intestine, bladder, or strong parchment, was even tainted with it.<sup>3</sup> He ascertained also that camphor, valerian, and mineral tar communicated their odour to water when it is exposed to the effluvia arising from these substances.<sup>4</sup>

Although it has not been demonstrated experimentally, it seems highly probable that contagious effluvia, like fetid emanations, are soluble in water, from the fact that thorough ablution of the clothes of persons who have laboured under fever disinfects them completely. Hence the advantage, as a prophylactic, of frequently sponging the skin of a typhus patient with water, more especially as tepid sponging is useful in the treatment of the disease. It appears, also, that contagious effluvia are volatile, like the emanations from putrid bodies, and may be separated from substances to which they adhere by means of heat. The late Dr. Henry of Manchester found that clothes impregnated with the miasmata of scarlatina and typhus were disinfected by exposing them to a temperature of 204° F. for one hour and three quarters, and that they did not induce any of these diseases when afterwards worn by healthy individuals.<sup>5</sup>

Are we then entitled to believe that butchers, candle-makers, &c. are more rarely affected with fever than other operatives? Dr. Tweedie supposes the exemption of butchers to depend on their good living; but it appears to us that the common theory respecting the operation of oily or greasy bodies in preventing fever will also explain the matter, and will apply to the butcher as well as to the tallow-chandler. It has already been shown by the experiments of Parent-Duchatelet, that greasy bodies attract powerfully putrid emanations; and it is well known that they unite very readily with odoriferous bodies of almost every kind; is it not therefore probable that contagious effluvia are regulated by a similar attraction, more especially when this hypothesis is coupled with the commonly received opinion in eastern countries, that oil is a prophylactic to contagion. If this be granted, how then does an oily or greasy body protect the butcher or the candle-maker? In the exercise of their various manipulations, the persons belonging to these two occupations have their clothes and the uncovered parts of their bodies more or less imbued with grease, an accompaniment which they almost constantly carry about with them. The contagious effluvia may, therefore, in place of being absorbed by the skin, combine permanently with the fatty body, and in this be fixed and rendered harmless.

We only bring forward this as an hypothesis capable of accounting for the generally received opinion respecting the protecting

<sup>1</sup> *Annales d'Hygiène Publique*, tom. v. p. 39.

<sup>2</sup> *Ibid.* p. 44.

<sup>3</sup> *Ibid.* p. 39.

<sup>4</sup> *Ibid.* p. 38.

<sup>5</sup> *Philosophical Magazine*, Nov. 1831.



property of oil; but certainly if there be prophylactic powers in it or in any other substance, it is well worthy of being investigated experimentally.

---

#### ON THE IDENTITY OF TYPHUS AND THE TYPHOID FEVER.

As we have made several quotations from M. Chomel, as well as from M. Louis, who seem to think that the typhoid fever of France is a different disease from the ordinary British typhus, it may be necessary to show, although it may appear foreign to this essay, upon what grounds we consider them identical. The evidence by which the identity of typhus and the typhoid fever may be established, consists of two kinds, namely, the symptoms during life, and the morbid appearances after death; and in order that the subject may not be entrammeled with unnecessary detail, those symptoms and lesions only which in the aggregate are reckoned diagnostic of the disease shall be described. M. Chomel describes the disease under three septennary periods, each being characterised by peculiar symptoms. First period is characterised by feebleness, stupor, sleeplessness, mutterings, meteorismus, diarrhœa, sensibility of the abdomen, and a sense of fluid gurgling in the lower half of the belly, epistaxis, the typhoid eruption, and frequent pulse. Second period is characterised by the eruption which M. Chomel admits to be similar to that described by Hildenbrand, as observed in the *typhus castrensis*, sudamina, ulcerations and sloughs on various parts, chops and ulcers in the tongue, increased stupor, unconsciousness, dorsal decubitus, difficulty of deglutition, involuntary evacuations, retention of urine, subsultus tendinum, picking of the bedclothes, general and permanent rigidity of the members, deafness, coma, small weak tremulous pulse, or throbbing and intermittent, and varying in frequency from 80 or 90 beats to 120 in a minute, but which sometimes sinks to 40 or 50, a fuliginous coating of the tongue, teeth, gums and lips, diarrhœa, intestinal hemorrhages, increased meteorismus, respiration more constrained, fetid exhalations from the skin and breath. Third period. It is generally during this stage that the febrile disorder subsides, whether the patient recovers or dies. When the termination is going to be favourable the patient becomes more sensible, is more disposed to sleep, the mouth and tongue become more moist, the fecal discharges more natural, and the pulse becomes less frequent. On the other hand, when the termination is going to be unfavourable, the stupor increases, there is an alteration in the features, stertorous breathing, feebleness of pulse, a drier skin, or cold and covered with clammy sweat, general emaciation, hollow eyes, tremulous speech, indistinct and murmuring answers to questions,

extreme feebleness, coma, and death. Sometimes death is accelerated by the occurrence of tetanic or epileptic paroxysms, and intestinal perforations and erysipelas are mentioned as occurring during convalescence. Any practitioner who has paid close attention to the symptoms of British typhus will readily discover their identity with those so well described by M. Chomel, as indicating the typhoid fever. There are, however, too or three symptoms which he places more dependence upon as characteristics of the disease than what is generally done in Britain, which it is necessary to notice more particularly. He represents diarrhœa as a very common symptom in the majority of cases, there being from four to eight alvine evacuations daily. Now this symptom by no means occurs frequently in Britain, but this discrepancy may, to a certain extent, be explained, for the French physicians seldom exhibit purgatives in case of aggravating the *gastro-enterite*; hence the solid excrementitious matter which naturally accumulates in the torpid bowels of a typhoid patient will produce a morbid secretion from their excited surfaces, and being tinged with feculent matter may represent a fecal diarrhœa. This view is supported by the admission of M. Chomel himself. He states that "in some cases, at the time when the first improvement in the symptoms occurs, the alvine evacuations consist of firm, figured motions, to the great astonishment of the attendants, who with difficulty understand how such a change could be effected in so short a time. It is probable that these matters had remained during the whole period of the disease in some of the cells of the colon, and had not prevented the passage of liquid motions. There are discharged sometimes in these cases prodigious quantities of black dry matters." From M. Chomel's account it would appear that meteorism or tympanitic swelling of the belly is more frequent in France than in Britain, for it has never been considered in this country as peculiarly characteristic of typhus. This discrepancy may, however, be reconciled, for according to this author the meteorism is only to be discovered in the early stages by percussion, while in the latter stages it is discoverable from the convex form of the belly. British practitioners apply the term tympanitis only to prominent distention of the belly by flatus, while those in France apply it not only to this but to minor enlargements not discoverable by the eye. Epistaxis is another symptom which M. Chomel considers frequent, and of great value as a diagnostic of typhus, especially if it occur during the first days of the disease. These hemorrhages are not profuse, but are most generally only a few drops, either from the anterior part of the nasal cavities or from the posterior by the throat, in the form of mucous masses, streaked and mixed with blood. Bleeding from the nose or mouth is certainly not so frequent in Britain as to constitute a diagnostic

<sup>1</sup> Chomel, *Leçons de Clinique Médicale*, tom. i. p. 42.

symptom of typhus, although it does occasionally occur; but it is generally hemorrhage to a considerable extent which has been noticed by authors in this country, and we do not doubt that the smaller discharges of blood or bloody mucosities have occasionally been overlooked or not attended to, as unimportant. M. Chomel, although he does not appear to be perfectly convinced that typhus and typhoid fever are the same disease, is strongly inclined to this opinion from the similarity of their symptoms. He says that "another point which is still in favour of the opinion of contagion is the analogy which exists between the typhoid affection and typhus of camps, the contagious character of which is contested by no person. If we compare these two diseases, and from our recollections and from the description which has been given by Hildenbrand, and which it was in our power during 1814 to verify the accuracy, we shall find the same symptoms in the two affections, both of them commence by headach, with most subjects prostration and stupor appear at the beginning, and not solely, as in other affections, after the malady has endured a long time, and has very greatly debilitated the organism. The other symptoms, such as the meteorism, the diarrhœa, the notable weakness of the senses, the tendency to ulcerations and hemorrhages, are common to the two diseases. The progress is the same in the two diseases, inflammatory symptoms predominate at first and are afterwards followed by nervous or adynamic phenomena. One of the few differences which we have observed between these two affections consists in the duration, which is more prolonged in the typhoid affection than in typhus. This last ceases generally about the fourteenth day, whilst it is rare that the first terminates before the twentieth day. Another difference consists in the frequency with which true petechiæ or purple spots are observed in typhus, which are comparatively rare in the typhoid malady. With regard to the cutaneous exantheme or typhoid eruption, it presents the same characters in the two affections; the only differences are in the number of spots and in the period of their appearance. In place of being confined, as they are most frequently in the typhoid fever, to the belly and chest, the lenticular spots in typhus cover and in greater numbers almost the whole surface of the body. In this last the eruption is developed generally about the fourth day of the disease; in the typhoid fever it appears only about the eighth day, and sometimes much later. . . . The only difference which Hildenbrand and Pringle admit between typhus and the most of other fevers which we have referred to the typhoid malady, is that the severity of the disease is greater in typhus, its progress more rapid, the adynamic phenomena more decided, and the eruption more general; but these differences are not sufficient to make us reject the identity of the malady, for they may depend upon circumstances more or less troublesome, during which it is propagated. These



differences may rather indicate degrees of intensity than that they are maladies entirely distinct."<sup>1</sup>

These distinctions between typhus and typhoid fever, as stated by M. Chomel, must appear to every one sufficiently acquainted with the typhus of Britain as very unimportant, for in young persons the eruption is frequently observed upon the extremities as well as upon the breast and belly, and even in the same family, when the disease ought to be acknowledged as identical, the number of spots observed on each member of it often varies exceedingly. It is also a well-known fact that complete convalescence from typhus fever rarely takes place on the fourteenth day except in young persons; while among those more advanced twenty or a greater number of days may elapse before this occurs.

In order to show still further the identity of the symptoms of typhus with the typhoid fever, we shall quote the observations of a very accurate and experienced physician, Dr. Lombard, of Geneva. He states that "with this experience and having witnessed numerous dissections of subjects dead of typhus fever, and having found in every one of them at Paris and at Geneva the morbid state of the intestinal canal which the French pathologists consider as essential; under these circumstances, when I arrived in Great Britain and had an opportunity of seeing the fever cases here, and when I found that they presented a very great similarity, if not an identity, of symptoms with those I had been for years in the habit of observing, it is not to be wondered at, I say, that I should have expected to find exactly the same post-mortem appearances. I mentioned this subject to my friends at Glasgow, and they allowed me to dissect the body of a person in whom I said no doubt could exist as to the presence of follicular disease; judge then, how great was my astonishment at not being able to detect a single trace of this morbid change in any part of the intestinal canal, and at finding no marks of disease save some redness and softness of the mucous membrane of the stomach, which may have been produced by inflammation, but more probably was owing to muscular congestion, occurring during the last stage of the disease, or even during the agony that precedes death."<sup>2</sup>

Dr. Lombard, however, was not convinced by this inspection; and on his arrival at Dublin he examined the bodies of two patients who had died of typhus at different hospitals, and with the same results. It thus appears that the symptoms of typhus and the typhoid fever are nearly the same, and that they cannot be distinguished from one another; so that upon this ground their separation cannot be maintained. But those who support the difference of the two affections rest their proof chiefly upon the pathological lesions which are found in the intestines.

M. Louis characterises the typhoid fever under the following

<sup>1</sup> Chomel, *Leçons de Clinique Médicale*, tom. i. p. 335.

<sup>2</sup> *Dublin Journal of Medical Science*, vol. x. p. 18.

description: "An acute malady accompanied with a febrile movement more or less intense, variable in its duration, proper to young persons, chiefly to those who are placed within a short time in circumstances new to them, the cause of which is unknown, commencing by a violent shivering, anorexia, thirst, and in the great majority of cases by colics and diarrhœa, very soon accompanied by feebleness which is small in proportion to the other symptoms, then more or less quickly somnolence, stupor, delirium, meteorismus, sudamina, lenticular rose-coloured spots, ulcers on the sacrum, ulcerations more or less deep of the skin, in the parts occupied by blisters, deafness, various spasmodic movements, or permanent contraction of the limbs; symptoms some of which disappear after a certain time, others increase for the most part in a progressive manner, when the patients die, or diminish more or less rapidly, at length to disappear altogether if the affection has a happy termination; the anatomical characters of which consists in a special alteration of the elliptic plates of the ilium.<sup>1</sup> . . . . . Of all these lesions one only is constant, being found in all the subjects: I speak of the alteration of the elliptic plates of the small intestines, to which may be added the alteration of the mesenteric glands; I have regarded it as inseparable from the existence of the affection under review in forming the anatomical character. And as it was more or less great with some subjects who died on the eighth day of the disease, as with the greatest number the first symptoms indicated a lesion of the intestinal canal, as the alterations of the small intestines was greater than those of the colon, which was sound in a sufficiently large number of cases, I am warranted to conclude that the alteration of the elliptical plates commenced at the beginning of the disease."<sup>2</sup> M. Chomel, although he appears strongly inclined to support the doctrines of M. Louis and the other French pathologists, makes the following candid avowal of his opinion deduced from a rigid examination of all the pathological facts connected with the typhoid fever: "If, to this consideration furnished by analogy, we join these two other circumstances already established: 1st, that there is no constant proportion between the severity of the symptoms and that of the lesions of the follicles; 2d, that the lesion has been completely absent in subjects who had offered during life all the symptoms of typhoid affection—it will become still more evident that the typhoid malady does not consist essentially of inflammation of the follicles; that this inflammation is only one of the phenomena of the disease, that it belongs, like most of the disseminated inflammations, to secondary inflammations; that it may be compared as to its pathogenic power not even to the pustules in variola, for in this there is always a proportion between the number of the pustules and the severity of the malady, but rather to the bubo in the pestilence of the East."<sup>3</sup>

<sup>1</sup> Louis de Gastro-Enterite, tom. ii. p. 317.      <sup>2</sup> Ibid. tom. i. p. 449.

<sup>3</sup> Chomel, Leçons de Clinique Médicale, tom. i. p. 536.

M. de Claubry, in his prize essay read before the Royal Academy of Medicine, has adduced very copious evidence to prove the identity of typhus and the typhoid fever. He controverts the opinions of M. Louis respecting the ages that are exempt from the typhoid fever, and states that "it is not rare to see the disease in the Parisian hospitals at the age of four, six, eight, and ten years; and that M. Andral has witnessed it after seventy years."<sup>1</sup> He adduces Fauvages, Reveillé, Parise, Thruvenel, Ducastaing and Pellerin, to prove that ulcerations having elevated borders and exposing the peritoneal coat were found near the extremity of the small intestine in typhus.<sup>2</sup> The same author also shows that the typhoid fever spreads by contagion in the same way and under the same circumstances as typhus.<sup>3</sup> Dr. Lombard, who contends for the distinction of the two diseases, adduces similar evidence to prove that typhoid fever is possessed of contagious qualities.<sup>4</sup>

We think it unnecessary to adduce evidence to prove that the follicular disease of the intestines is greatly less frequent in British typhus than in the continental typhoid fever; for the pathological investigations which have been made in England, Scotland, and Ireland, regarding this point, are now numerous and well known. Indeed in this country, in place of finding in almost every subject who died of typhus fever disease in the agminated or solitary glands, the minority has been the proportion found in many hospitals, and the affection of the spleen and brain more frequent than that of the intestines. If then there be no specific difference between typhus and typhoid fever; why are the pathological lesions of the intestines so much more common and intense in France than in Britain? It is perhaps not possible to give a satisfactory answer to this question, unless a difference of climate, diet, habits, &c. be allowed a certain influence. Dr. Lombard, in his first letter to Dr. Graves, seemed to have formed a very correct opinion respecting the nature of typhus, although he afterwards thought proper to change his views. In his first letter he says that "all these considerations, my dear friend, seem inevitably to lead to the conclusion that typhus fever is more a general disease affecting the whole constitution than a malady depending on a local inflammation or any local change of structure. May we not infer, also, that various causes serve to impress upon this general disease a tendency to associate itself with and produce various local ailments; among these causes, the most influential probably are, climate, seasons, the race of mankind, diet, and various circumstances which act powerfully both on the mind and body, and which when concentrated at any one point of time have given rise to those various epidemics of typhus that have so frequently devastated the different

<sup>1</sup> Mémoires de l'Académie de Médecine, vol. vii. p. 190.

<sup>2</sup> Ibid. p. 80.

<sup>3</sup> Ibid. p. 120.

<sup>4</sup> Lombard's Clinical Remarks on Bilious and Typhoid Fevers, p. 17.



countries of Europe."<sup>1</sup> The same author, however, in his second letter to Dr. Graves, assumes his old hypothesis that the two fevers are different, and goes even a step further, for he maintains that both kinds are to be met with in the British and Irish hospitals. His views seem to be included in the following quotation from his letter: "But the Irish contagious fever is not the only source of typhoid diseases in Great Britain; the sporadic continued fever, observed in all parts of Europe, is also to be found in the different towns of the British empire. This fever, characterised by the follicular intestinal eruption and by consequent ulcerations, is to be seen in the different places above mentioned; in Glasgow it forms one third of the total number of cases;<sup>2</sup> in Dublin the proportion is much less; in London it is one fourth, and varies in the different seasons, because the continued sporadic fever is much under the influence of the temperature, being more frequent in autumn than in spring and winter; a proof that the proportion of this sporadic fever is the cause of the greater proportion of ulceration cases found at times in the British hospitals, as already mentioned. Having stated my opinion on your British continued fever, I resume it in the following theoretical view: You have two different fevers, one highly contagious, which I may call the Irish typhus, and in which the cephalic symptoms predominate to the exclusion of abdominal alterations; the other which is sporadic and most likely not so infectious, and in which the abdominal symptoms are more predominant, so much so that the follicular disease and consequent ulcerations are always to be found."<sup>3</sup> Dr. Gerhard, of Philadelphia, is another author who endeavours to show that there is a specific difference between typhus and typhoid fever, and that both are to be met with in Philadelphia. He makes the following observations respecting the post mortem appearances which were observed in the American typhus: "In this large number of autopsies, amounting to about fifty, there was but in one case, and that doubtful in its diagnosis, the slightest deviation from the natural appearance of the glands of Peyer. In the case alluded to, in which there had been some diarrhœa, the agglomerated glands of the small intestines were reddened and a little thickened, but there was no ulceration and no thickening or deposit of yellow puriform matter in the submucous tissues. The disease of the glands resembled that sometimes met with in smallpox, scarlet fever, or measles, rather than the specific lesion of dothionenteritis."<sup>4</sup>

<sup>1</sup> Dublin Journal of Medical Science, vol. x. p. 23.

<sup>2</sup> In some places in Scotland ulceration of the intestines seems to be very frequent. Dr. John Reid states that Dr. Goodsir, of Anstruther, examined ten bodies, and in every one the elliptical patches of Peyer and the solitary glands at the lower part of the ilium were elevated and ulcerated, and in four, perforation of the intestines had taken place. Edinb. Medical and Surgical Journal. Oct. 1839, p. 459.

<sup>3</sup> Dublin Journal of Medical Science, vol. x. p. 104.

<sup>4</sup> American Journal of Medical Sciences. February, 1837.

Dr. Gerhard's account of the epidemic typhus in Philadelphia is written with great accuracy, and his post mortem inspections seem to have been conducted with much care and ability; but his results are certainly not what might be expected from a disease of the same nature as British typhus, which he describes it to be. For though we by no means believe that the lesion of Peyer's glands is a necessary concomitant of typhus, we are certainly supported by British observations when we state that there never were fifty consecutive inspections of typhus subjects made in this country without finding one decided instance of disease in the intestinal follicles.

It is quite evident that Drs. Lombard and Gerhard lay almost the whole weight of the diagnosis of typhus from the typhoid fever, upon the lesions of the intestinal follicles observed in the latter disease; for the almost identity of their symptoms during life are admitted; and is there any British practitioner that could distinguish those cases of eruptive typhus that had diseased follicles from those that had not? Again, it may be asked, what is the peculiar character of the diseased follicles, which constitutes the distinction between typhus and typhoid fever? In subjects dead of typhus fever which we have examined, the follicles are generally found with their margins only distinctly defined, but with little elevation or thickening of the subjacent textures, but such as to give a comparative opacity to the patch; when viewed with a magnifier, their surface presents irregular mammillated projections, bounded by corresponding depressions; sometimes there is only one patch, more frequently two or three, or a large irregular coalescence of patches at the ileo-cæcal valve; deep ulceration is not very common except in protracted cases; and occasionally there is the appearance of superficial ulceration. Now, if the anatomical and distinctive character of the typhoid fever be a morbid alteration of Peyer's glands, one single diseased patch, characterised by its defined margin, greater or less elevation and opacity, ought to constitute the disease as definitely as if there were twenty; just as small-pox is as essentially distinguished by twenty or thirty pustules as by several hundreds. If this be denied, where lies the line of separation?

Does it consist in a certain elevation of the follicles capable of admeasurement, in the deposition of a yellowish white or puriform matter in their subjacent textures, or in a certain amount of ulceration? But it may be argued that there are two species of fever in Britain, the one characterised by a peculiar disease of the intestinal follicles, and the other unaccompanied by any such lesion; and that some slight disease, characterised by a slight elevation and configuration of the patches, does sometimes take place in the latter, such as occurs in scarlet fever, small-pox, &c.; but that this lesion is totally different from that described by Louis and Chomel as characteristic of the typhoid fever. Now, we are ready to admit, at least as far as our experience goes, that the elevation and texture

of the follicles are not in many cases precisely similar to those which are stated to be characteristic of the typhoid fever; but certainly they are even in this state morbidly affected.

The following table shows the lesions that appeared on the inspection of sixty-three eruptive cases, that were admitted into the Glasgow Fever Hospital from 1st May to 1st November, 1839, and it includes both male and female patients in nearly equal proportions:

|   |    |
|---|----|
| Abnormal serum in brain . . . . .               | 34 |
| Bronchia red . . . . .                          | 25 |
| Spleen rather large and soft . . . . .          | 14 |
| Spleen large and pulpy . . . . .                | 30 |
| Peyer's glands enlarged 1 to 3 . . . . .        | 12 |
| Peyer's glands enlarged 3 to 6 . . . . .        | 14 |
| Peyer's glands enlarged 6 and upwards . . . . . | 22 |
| Solitary glands enlarged . . . . .              | 14 |
| No intestinal glands enlarged . . . . .         | 12 |
| Ulceration of intestines . . . . .              | 13 |

The mesenteric glands were almost uniformly enlarged when ulceration of the intestines was present, but very rarely in other cases.

Now it may be contended that this simple enlargement or figuration of the intestinal follicles is a different affection from that which occurs in the continental typhoid fever, and hence ought to have a different classification. Such an assumption would lead to an endless and very unphilosophical division, and obviously to the formation of three species of typhus, out of the various complications or appearances which are observed in the intestines; namely, 1st, typhus without any intestinal affection whatever; 2d, typhus with simple enlargement of Peyer's glands; 3d, typhoid fever complicated with the follicular affection described by M. Louis; for if one author distinguish a species by a peculiar morbid appearance of the intestinal follicles, another has the same right to form a second, if the affection of these glands, in a certain number of other cases, be denied a pathological similarity to the first; whilst the morbid affections of the spleen, the lungs, the brain, &c. might all be brought forward to increase the subdivision still farther. The strength of our argument, however, that typhus and typhoid fever are the same diseases modified by place, season, epidemic influence, and perhaps by circumstances not yet ascertained, lies in the fact, that it has been admitted that cases of the latter disease, although rare, have occurred without any morbid appearance being discovered in the intestinal follicles; proving that this morbid condition of these glands is not a necessary anatomical character of the disease, such as hepatization or suppuration is of pneumonia, or serum of hydrocephalus. It has also been admitted that the intensity of the symptoms is not proportional to the lesions which ought to occur if the latter were the cause of the former; and it would be contrary to all experience to attribute the formidable symptoms of



typhus or the typhoid fever to the lesion of one or two intestinal follicles, even though affected in the form described by the French writers. Would it not, therefore, be refining our classification of diseases beyond all precedent, to separate typhus and typhoid fever into two species, where it has been shown that the symptoms in both are the same, or very nearly so, that they have nearly the same laws, as far as these have been ascertained ; that the severity of the symptoms in both is not in proportion to the lesions of the intestinal follicles ; and that the other complications of both are similar, although various in the same places at different periods, while the only characteristic in dispute has been acknowledged not a constant and therefore not a necessary element for the existence of the disease.



## II.

### AN INQUIRY

INTO THE SOURCES AND MODE OF ACTION OF THE

### POISON OF FEVER.

---

BY ALFRED HUDSON, M. B. T. C. D.

PHYSICIAN TO THE NAVAN FEVER HOSPITAL.

---

Much as has been written upon the history of fever, it cannot by any means be considered as an exhausted subject. If indeed we were to test our knowledge of its sources by the universality of their admission, and consider the general agreement of all observers as to their laws as the true proof of these being fully ascertained—a criterion which is applicable to medicine as to the other sciences of observation—we should see reason to conclude that in reality our knowledge of the causes of fever and their modes of action upon the living body is of very small account, and by no means of the most accurate description; for though, in this country at least, the doctrines of localisation of fever are not advocated, nor fever considered the effect of inflammation of any particular organ or organs, we find in the most recent writers, equally as in the ancient, the widest differences of opinion as to the phenomena which constitute the origin or nature of this *essential disease*.

An analysis of the mass of conflicting statements upon this subject may perhaps be useful, if only as a preliminary step to other inquiries, by showing how much of what has been put forward as evidence is really founded on observation, and how much is on the contrary mere matter of opinion and not of fact. Such an examination of what has been advanced upon the disputed question, it is proposed to attempt in the following inquiry.

We regard the essential disease termed fever as the effect of the action on the living body of a morbid poison—in other words of—

“One of that peculiar class of substances which are generated during certain processes of decomposition, and which act upon the animal economy as deadly poisons; not on account of their power



of entering into combination with it, or by reason of their containing a poisonous material, but solely by virtue of their particular condition."<sup>1</sup>

The mode of operation of this poison upon the body is a fertile theme for disputation between the humoralists and solidists of this as of preceding ages, and whence it is derived and where generated—whether in the body or out of the body—the contagionists and non-contagionists are as much disagreed about as ever.

The humoralist holds that the very definition of a morbid poison, if correctly given by the distinguished author from whom we have adopted it, would point to the blood as the subject of its operations. Since, assuming that the *essence* of such poison is that its elements are in a state of decomposition or transposition—and its *action* to communicate that peculiar transposition to the constituents of the body with which it may be brought into contact, *he* finds in the blood a substance the most susceptible of any part of the organism of the action of exterior influences, and whose constituents are the most prone of any to form new combinations. The humoralist also points to the analogy of other morbid poisons, which produce their specific effects upon their direct introduction into the blood. He points to the latent period common to both; and, if he be a contagionist (as he must be,) he sees in the formation of the poison by the blood the consequence of the introduction of organic matter in a state of progressive transposition or decomposition (such as is the contagious miasm) into a mixed fluid in which its constituents are contained, and the reproduction in that fluid of the exciting body, exactly as yeast is reproduced when added to a mixed fluid in which the gluten from which it originated is contained. On the other hand, the solidist considers that the nervous system is so much engaged in fever, that the poison must be there, or, the phenomena of the latent period are attributable to the nervous system, or, dating this commencement of fever from the nervous shock, sometimes attendant on exposure, and *assuming* that the poison is received into the organism *then* and *there*, he sees an analogy between the action of the poison and certain narcotic substances which he *assumes* act on the nervous system without entering the circulation;—and therefore—fever so acts—or—lastly, the *source* of the poison not being apparent, and the shock preceding the fever, he finds that he can produce contagious fever by a moral impression on the nervous system without the action of a poison at all! These are some of the different opinions maintained by recent and able writers on the nature of fever, and which we shall have to glance at when considering the mode of action of the sources of the disease. As to these sources, our latest writers are so disagreed as to make an analysis of their opinions and evidence no easy matter. If we placed them in juxtaposition

<sup>1</sup> Liebig's Organic Chemistry.

according to the doctrines propounded and denied, our index would run thus—

Fever contagious—not contagious.

Arises from putrefying animal matter—denied.

Arises from putrefying vegetable matter—denied.

Infection a direct emanation from the patient—denied.

Infection capable of being generated *de novo*—denied.

Atmosphere of patient infectious—denied.

Contact of ditto infectious—denied.

Fomites infectious—denied.

Fever originating in miasm contagious—denied.

Identity of foregoing with typhus asserted and denied.

These conflicting opinions will come under review successively in the course of an examination into the following questions.

1. The existence of a special animal poison arising from infection, and producing a specific disease—typhus.

2. The generation during the decomposition of organic substances of a poison capable of producing fever when applied to the living body.

3. The power of this paludal fever to communicate itself from one individual to another. Does it possess the power of infection *per se*? in other words, are typhus and typhoid fevers identical? or, does it acquire it by the aid of adventitious circumstances, and so become communicable by conversion into or superaddition of typhus?

4. Arising out of the preceding is the inquiry—what are the adventitious aids to the diffusion of each kind of fever? the laws which regulate their epidemics, and the sanatory measures best calculated to neutralise their operation.

## CHAPTER I.

Of the Infectious Animal Poison generated in the living Human Body, and capable of producing Fever when applied to Healthy Bodies.

SECT. 1.—*Proof of its tangible existence.*—It might have been supposed that the accumulated evidence of infection presented in the histories of the typhus of Great Britain, would satisfy the most incredulous; but it is not so, and a recent author has denied the existence of such a cause of fever as atmospheric contagion<sup>1</sup>—in other words, of “an atmosphere holding in solution a specific contagious poison.” Because—“it has never been unequivocally manifested to any of the external senses; it has never been seen combined with the atmosphere, or precipitated from it, or abstracted therefrom to solid bodies.”

It has been urged in reply, that this is equally the case with miasm and vitiated air of all kinds, which last, this author himself

<sup>1</sup> Dr. Scott Allison—*Essay on Contagion.*

has endeavoured to prove, is the source of contagious fevers. But this answer is not correct, the fact being, that organic matter in a state of decomposition, or progressive transformation, is present in both. We shall hereafter adduce evidence of this fact with regard to miasm. As to its presence in aerial contagion Liebig states that—"all the observations hitherto made upon gaseous contagious matters prove that they also are substances in a state of decomposition. When vessels filled with ice are placed in air impregnated with gaseous contagious matter, their outer surfaces become covered with water containing a certain quantity of this matter in solution. This water soon becomes turbid, and in common language, putrefies; or, to describe the change more correctly, the state of decomposition of the dissolved contagious matter is completed in the water. The odour of gaseous contagious matters," says the same author, "is generally accompanied by ammonia, which may be considered in many cases as the means through which the contagious matter receives a gaseous form. . . . . Ammonia is very generally produced in cases of disease; it is always emitted in those in which contagion is generated, and is an invariable product of the decomposition of animal matter. The presence of ammonia in the air of chambers, in which diseased patients lie, particularly of those afflicted with a contagious disease, may be readily detected; for the moisture condensed by the ice in the manner just described, produces a white precipitate in a solution of corrosive sublimate, just as a solution of ammonia does. . . . By evaporating acids in air containing gaseous contagions, the ammonia is neutralised, and we thus prevent farther decomposition and destroy the power of the contagion, that is its state of chemical change."

"To this decisive proof of its presence may be added the fact of its being frequently recognised by one of the senses, that of smell, in those cases, in which it has proved active as a poison. For instance—a gentleman in this neighbourhood, not long since, passed through a severe and lengthened typhus fever. About the tenth day of convalescence, while walking across the room, leaning upon the arm of his son, the latter was struck by the odour from his father's body; he immediately became sick at stomach, and on the next day had rigour followed by fever of the same type and duration (21 days) as his father's."

Dr. Montgomery's<sup>1</sup> attack of fever, related by himself, gives similar proof that the aerial contagion may be occasionally recognised by this sense.

"On the 10th of August, I visited a patient in fever, and hearing from the nurse that there were spots on the patient's skin, I stooped very close to her to satisfy myself, and while so doing, I was sensible of a very disagreeable odour from the skin. At the moment, it made a considerable impression on the sense of smell, being almost as pungent as the odour from an ammoniacal salt. The smell continued in my mind all day, &c."

<sup>1</sup> In Marsh's Essay on the Origin of Fever.



It is true a sceptical reasoner might argue for the possible existence in such cases of an unhealthy locality, impure air, &c. ; but much of the evidence of contagion which we possess, is free from any such objection.

SECT. II.—*The Testimony in proof of the Power of this Poison to cause Fever*—or, as it may be expressed, the proof that the disease has arisen from exposure to the emanations from the bodies of those similarly affected, requires to be of a very exact kind, since the opponents of the doctrine of infection, who, like the writer above quoted, affirm, that “those who have communication with the sick do not suffer in a greater proportion than those who keep apart,” explain the many instances in contradiction of this assertion which occur among the medical attendants, nurses, and relatives of the sick, by attributing them to the “locality” and to “impure air,” and add, that “it is however almost entirely on such exceptions as these that the contagionists depend for the maintenance of their gloomy doctrines.”<sup>1</sup>

The great weight of the proof derived from the experience of the large fever hospitals in England, Ireland, and Scotland, has been well shown by Drs. Tweedie,<sup>2</sup> Alison,<sup>3</sup> Christison,<sup>4</sup> and Davidson,<sup>5</sup> and the last gentleman justly observes, that “the simple relation of these facts would, with the majority of men, produce conviction that fever was at least contagious in these hospitals, provided the mind was not pre-occupied with an opposite theory.” Certainly none but a determined anti-contagionist could resist the fact, that, in the large fever hospitals of the three countries, every clerk has, during some period of his attendance, laboured under fever.

It is also ably proved by Dr. Christison, that the proportion of attacks among attendants is in the ratio of their exposure to the emanations from the sick. It being observed that, in the Edinburgh hospitals, they were affected in the following order as to frequency. 1. Nurses. 2. Resident clerks or house-surgeons. 3. House servants. 4. Medical students not attached to the service of the institution. Thus, in the epidemic of 1818, of thirty-eight nurses, only two or three escaped. Of the fifteen gentlemen who filled the office of resident clerk between 1817 and 1820, only two escaped.

But, overwhelming as this argument from hospital experience appears, some have considered it open to objections.<sup>6</sup> It has been

<sup>1</sup> Medico-Chirurgical Review, vol. ii. New Series.

<sup>2</sup> Clinical Illustrations of Fever, and Art. Fever in Cyclop. of Pract. Med.

<sup>3</sup> Essay on the State of the Poor in Scotland.

<sup>4</sup> Library of Medicine. Article, Fever.

<sup>5</sup> Thackeray, Prize Essay.

<sup>6</sup> Dr. Fergusson, Edinburgh Med. and Surg. Journal, No. 112. See also a Discussion at the Royal Academy of Medicine, reported in the Medico-Chirurgical Review, Jan. 1839, in which the opinion was advocated by MM. Rochoux and Chervin, “that the disease is not communicable directly from one person to another, but is only transmissible in the way of infection,

argued, that the typhus thus received (or rather the infection of typhus) is *factitious*, and created by causes over which we ought to have exerted due control; "that the poison can only be made effective through contamination of atmosphere under long-continued accumulation of morbid effluvia; and, in fine, that the atmosphere of the patient is infectious, and not his person." This argument receives some support from the experience of large general hospitals, which, particularly in London and Bristol, give admission to cases of typhus without its ever being observed to spread; and, from the acknowledged rarity of communication under the closest approximation among the better classes of society. It is said also that M. Louis never saw a case of communication of fever in an hospital, and Dr. Elliotson states that he never saw a case of fever infectious. It may however be urged in reply, that the observations of the latter eminent observers apply to a different fever—an endemial; and that the argument proves no more than that the infection of typhus is weak, compared with other infections.

But there is another kind of evidence, scarcely less decisive than that derived from the records of the large hospitals. It is thus somewhat flippantly disposed of by Dr. Davidson.

"In the outset it may be stated that we do not mean to fatigue the reader by stories about fomites, and persons who have carried the contagion about them for months or years, nor to hunt out a particular individual who has conveyed it from one town to another," &c.

Now we think it is an admirable rule "as laid down by Dr. Elliotson,"—"That for infection to be *proved*, the individual who communicates the disease must go from the place where he resides to the spot where the healthy person is, and there give it to the latter. If the healthy person go to the sick person, and the sick person be still in the place where he was living when attacked, then no one can say that the disease which the former contracts has not been produced from the *situation*, and not from the *patient*. The disease may have arisen from contagion—from the emanations of the patient—but this is not proved. . . . Whenever such a thing occurs as disease being produced in a healthy spot by the approach of an unhealthy person to a healthy one, or by the application of fomites to a healthy person, then it is a proof of contagion, provided the instances be sufficiently numerous, for one or two cases may be quite accidental."

The following is a fair case of importation by both person and fomites:—"A beggar from Limerick obtained admission into a labourer's cabin for herself and a dying child. In five days after she quitted the cabin fever took place in one of the family, which consisted of a man, his wife, and five children, and in succession, within a day or two of each other, every individual sickened, and when the atmosphere around becomes loaded with the miasms which exhale from the bodies of the sick."

<sup>1</sup> Lectures by Rogers.

two children from a neighbouring cabin, who had attended the child's wake, took the same fever within ten days after, and communicated it to their family. The beggar (*herself in good health*) went to a farmer's house two miles distant, and obtained a lodging for the night, after her child was buried—every individual in the family (five in number) also took the fever within a few days—these fevers were all severe.”<sup>1</sup>

That these cases of importation of infection occasionally exercise a very great influence in the spread of fever, we are convinced by our own hospital experience. From the middle of the year 1834 to the same period in the year 1836, scarcely a single case of *contagious* typhus was admitted into the Navan fever hospital. The entire number of *fever* cases only amounting in that time to 363, and these being all instances of epidemic gastric fever or endemial typhoid fever. In the month of July, 1836, three cases of a *new fever* were admitted together. On inquiring their history, I was told that one of them was the seventh of his family who had been attacked—the other six having died. The two men who were admitted with him came from the same neighbourhood—seven miles distant—and had both had communication with the infected family. These were cases of typhus with measly efflorescence, profound adynamia, delirium, &c.

About two months afterwards an elderly man, with six of his family, were admitted labouring under typhus fever. They were from an opposite direction, about seven miles distant. The fevers, of which these were the commencing cases, spread rapidly and widely, and such was their effect upon our admissions, that the number of fever patients increased from 363 in two years to 400 in 1837, and 600 in 1838; when they were at their height. While these cases continued distinctly marked, and differed so much from our ordinary endemial fevers, as to be recognised at once by the nurses, as well as to be dreaded from their greater fatality and until they became merged in the epidemic of the past year, our cases of typhus were nearly exclusively derived from the districts in which these originated, or to which they had spread.

Careful inquiries were made as to the source of the epidemic in each case, and the following particulars were ascertained.

It appeared that in the first, a man had arrived in this country from America. It was stated that the voyage had been an unusually rapid one, and he had been ill the whole, or nearly the whole time. On landing, he was immediately removed to his father's house, twenty miles distant, and on his arrival there, was seen by a medical man, who pronounced his disease to be fever. He died on the second day after his arrival.

His father's house and neighbourhood was previously quite healthy; but in two days after his death, the father sickened, and, on the day following, his sister. She communicated the disease to

<sup>1</sup> Barker and Cheyne's Report, vol. ii.



her husband, who lived half a mile distant. He was attended by his brother, who caught the disease, and was one of the three first brought into hospital. The father was visited before his death by a brother, residing nearly two miles distant—on his return home he sickened, and in the course of his illness communicated it to his son. A brother of the importer contracted the disease, (apparently from his father,) and was sent into hospital, where he died, as did all the above, with the exception of the one who was sent to hospital. In short, of the family of the importer, eight out of nine were infected, and seven died.

In the course of a short time several other families, we have been informed, were completely exterminated. It spread with a rapidity and fatality perfectly unprecedented and long maintained its hold in the town and neighbourhood.

We, of course, have no means of determining the source from which the original case was derived; but we were much struck on meeting with Dr. Gerhard's account<sup>1</sup> of the typhus which prevailed in some parts of America in that year, with the resemblance between this epidemic and that which he has so well described—especially in the acrid infectiousness of both.

We have not to *hunt* so far for the second case.

This man's daughter was a servant in Dublin, where she contracted typhus and died. Her brother went to see her, and remained till her funeral took place. He sickened—came home, and died of what was described to me as a long, spotted, fever.

After his death, the abovementioned seven persons sickened within a day or two of each other, and were sent to hospital. The father died; several of the others had very severe fever; typhus spread from this house, first to the immediate neighbourhood, and subsequently to the surrounding country.

These instances are by no means all of the kind that have occurred within the time mentioned, but are selected on account of their wider influence and the unequivocal nature of the testimony they afford to the infectiousness of fever.

Among the most unquestionable sources of fever, is the communication of it by fomites carried from the patient to some place previously healthy.<sup>2</sup> It is in this manner that washerwomen frequently become the subjects of fever. Dr. Twcedie says, "to show that the disease may be engendered by fomites in clothing, the laundresses, whose duty it is to wash the patients' clothes, are so invariably and frequently attacked with fever, that few women will undertake this loathsome and frequently disgusting duty."

<sup>1</sup> American Journal of Medical Sciences, February, 1837, and Dublin Medical Journal, July, 1837.

<sup>2</sup> Dr. Stark's Experiments on the Power of different Colours to absorb Odorous Particles, (Edinburgh Philosophical Journal, April, 1834,) show that woollen substances constitute the most powerful fomites.

Dr. Armstrong,<sup>1</sup> an anti-contagionist, had previously noticed the same fact.

Dr. Reid and Dr. Cheyne,<sup>2</sup> inform us that, during the epidemic of 1817, not a single person of those appointed to receive the clothes of the sick escaped the disease.

The preventive effects of an early removal of the sick is one of the strongest proofs of infection, since the same measure produces no such effect in the endemial fevers.<sup>3</sup> The effect of early removal of the sick and the cleansing and whitewashing of their apartments, was very remarkable in checking the progress of the disease in some families, while, from the neglect of these precautions, the number of the sick rapidly increased in others. Two neighbouring houses, in Barrack-street, afforded an illustration of this remark, viz. Nos. 41, and 47. In the former the disease began in two different families, and its progress was immediately checked by early removal, cleansing, &c. In the latter, the individual first affected remained at home and died of the fever, but not before he had communicated the disease to eighteen persons in a short time.

On the effect of early removal of the sick, Dr. Alison<sup>4</sup> remarks, we should have little difficulty in pointing out above a hundred houses where a single case of fever has occurred, where the patient had been removed speedily, and the place cleansed, and where there had been no recurrence, &c. Dr. Ferriar<sup>5</sup> states, "that formerly, when a fever began in the Manchester Infirmary, it was found necessary to dismiss almost all the patients. . . . but since a few rooms were built in 1792, separated from the rest of the wards, for the reception of such cases, though the infection has been more than once introduced, yet by removing such patients as showed symptoms of fever at their first appearance into the secluded ward, and preventing all communication between them or their nurses, and the other patients and servants, the complaint has been stopped; and no reason has again occurred for a precipitate discharge of patients."

But in applying these facts to the proof of the infectious *nature* of fever, we are again met by the argument, that under all these circumstances of crowding, &c. they only prove that a factitious atmosphere of contagion is produced, and the anti-contagionist points to instances of typhus received into the Bristol and other hospitals, and mingled among the other patients without ever spreading the disease.

Dr. Davidson's quotation from Prichard, supports this view as regards Bristol. "In St. Peter's the wards are very small, and the

<sup>1</sup> Lectures by Rix.

<sup>2</sup> Dublin Medical Transactions, vol. iii.; and Dublin Hospital Reports, vol. ii.

<sup>3</sup> Report of Inspectors of House of Industry, quoted by Cheyne. Dublin Hosp. Rep. vol. ii.

<sup>4</sup> Edinburgh Medical and Surgical Journal, vol. xxviii.

<sup>5</sup> Medical Histories, vol. ii.

beds were near each other—offensive smells often perceptible—and, under these circumstances, the disease manifestly contagious. In the infirmary the wards are lofty and well ventilated—here also the fever patients were dispersed among invalids of every description—no instance occurred of the propagation of the fever—none of the nurses were attacked, nor any of the patients infected, though lying within two feet of cases of typhus gravior.”

From the infectious form of the disease prevailing almost exclusively among the poor, it is difficult to obtain, in Ireland, a case not liable to the above objections. The following is perhaps as decisive, and as free from objection as may be.

In the month of March, 1839, an old man, with his son and daughter—all of them persons of cleanly appearance—and in comfortable circumstances—were admitted into the Navan Fever Hospital. The history which they gave of their seizure was, that another son, the only other member of the family, had contracted fever, by sleeping for two nights in a house eight miles distant, in which was a person in an advanced stage of the disease. On his return home, he lay down in a fever of twenty-one days. About the third day after his crisis, his father sickened—on the following day, his sister, and in a day or two after, his brother. A day or two before these persons came into hospital, a young man, a cousin of the parties, was admitted. He was one of a family of *ten* living near his uncle’s house. He *alone*, of this family, visited his cousin during his illness. His family showed their caution farther, by sending him into hospital early in the disease. He passed through the same fever (typhus, severe in all, and fatal in the old man,) as the others, but no one of his large family took the disease; and on inquiry, a year afterwards, I learned that they were all still free from fever.

SECT. III.—*Varieties in the Nature or Sources of the Poison.*—The dogma of Dr. Bancroft, that the contagion of typhus<sup>1</sup>—“The original work of our common Creator must have been continued in existence by the energies of a living principle, exerted successively in the different bodies through which it has been transmitted from one generation to another,”—has met with comparatively few supporters among late writers on fever. Elliotson,<sup>2</sup> Barker,<sup>3</sup> Roupell,<sup>4</sup> Perry,<sup>5</sup> and Davidson,<sup>6</sup> espouse this doctrine, but without adding in the least to the meagre facts upon which it is founded.

On the other hand, numerous observers assert the production of typhus under circumstances in which the existence of a fever poison derived from a person labouring under the disease, was out of the question; and therefore they have assumed “that certain physical and moral conditions may so act on the operations of the body

<sup>1</sup> On Yellow Fever

<sup>2</sup> Dublin Med. Transactions, vol. ii. p. 595.

<sup>3</sup> Dublin Medical Journal, Vol. 10.

<sup>4</sup> Lectures, by Rogers, p. 296.

<sup>5</sup> On Typhus.

<sup>6</sup> Prize Essay.



as to cause it to generate within itself that which produces the phenomena of fever, independent of any exterior poison."

Dr. Ferriar<sup>1</sup> thus enumerates the circumstances, under the combined action of which fever has been observed to arise spontaneously.

1. Want of fresh air.
2. A deficient or improper diet.
3. Want of cleanliness, and, chiefly, want of a proper renewal and change of clothes.
4. Anxiety and depression of spirits.

The second and fourth of these are probably the essential causes of the generation of the poison, and the others assist by producing its accumulation—as in typhus—the diseased emanations constitute the poison; which, however, is all but harmless, unless accumulated.

The following graphic sketch of fever, thus originating, is given by Dr. J. Hunter.<sup>2</sup> In the month of February, 1779, I met with two examples of fever in the lodgings of some poor people whom I visited, that resembled in their symptoms the distemper which is called the jail or hospital fever.

It appeared singular that this disease should show itself after three months of cold weather. Being, therefore, desirous of learning the circumstances upon which this depended, I neglected no opportunity of attending to similar cases. I soon found a sufficient number of them for the purposes of farther information.

It appeared that the fever began in all in the same way, and originated from the same causes.

A poor family, consisting of the husband, wife, and one or more children, were lodged in a small apartment, not exceeding twelve or fourteen feet in length, and as much in breadth. The support of these depended on the daily labour of the husband, who with difficulty could earn enough to purchase food necessary for their subsistence, without being able to provide sufficient clothing or fuel against the inclemencies of the season.

In order, therefore, to defend themselves against the cold of the weather, their small apartment was closely shut up and the air excluded by every possible means. They did not remain long in this situation, before the air became so vitiated as to affect their health, and produce a fever in some one of the miserable family. The fever was not violent at first, but generally crept on gradually, and the sickness of one of the family became an additional reason for still more effectually excluding the fresh air, and was also a means of keeping a greater proportion of the family in the apartment during the day. Soon after the first, a second was seized with the fever, and in a few days the whole family perhaps were attacked, one after another, with the same distemper. The slow

<sup>1</sup> Medical Histories, vol. i.

<sup>2</sup> Remarks on the Jail or Hospital Fever, Medical Transactions, vol. iii.

approach of the fever, the great loss of strength, the quickness of the pulse, with little hardness or fulness, the tremor of the hands, and the petechiæ or brown spots upon the skin, to which may be added the infectious nature of the distemper, left no doubt of its being the same with what is usually called the jail or hospital fever. It would appear there is no great power of infection in the body alone provided the air be not confined. Remarking on the exemption from this disease which warm countries enjoy, he says—"On the cold is the cause of the air being confined which gives rise to the poison, and thus, directly opposite to the opinions usually received, there is more danger of producing this disease in a cold country, and in a cold season of the year, than in a warm one."

A person exposed to, and living in the poisonous air, becomes feeble and irritable, his sleep is disturbed, his tongue is white in the morning, his appetite is impaired, and the smallest bodily exertion quickens his pulse and fatigues him. He will remain in this state for weeks together, without any formed attack of fever; yet another receiving the infection from him, shall suddenly be seized with a violent disease. In this manner it is, I much suspect, that prisoners brought into a crowded court often produced the most dreadful consequences, by disseminating the infection lodged in their clothes. An instance of this kind is given by Dr. Fordyce,<sup>1</sup> which deserves mention. Arguing for a distinction between this poison and putrefactive poisons, he says—"This is undoubtedly not the case, since infection has arisen from a person brought out of rooms in which numbers had been confined for several months, but kept clean from all putrescent matter, so that there was no particular smell or other sensible quality. In one case that came under the observation of the author, a person under such circumstances, from whom no peculiar smell arose, or any other sensible effluvia, communicated the infection to four others with whom he was carried in a coach for about half a mile, so as to produce fevers in all of them, which fevers were violent and fatal."

Dr. Ferriar properly includes *moral* causes—"because it is not proved that the mere confinement of the effluvia of clean and healthy persons, free from mental uneasiness can become poisonous. This view derives considerable support from the following remarkable case by Dr. Harty, of the origin of fever from a single person under such circumstances.

A gentleman<sup>2</sup> was suspected of having confined and ill-treated his wife. At length two gentlemen, one of them a clergyman, having obtained the necessary authority, visited the house, and examined every apartment for the wretched object of their humane search—at first in vain; but at length a small closet door attracted their notice, and having insisted on its being opened, both gentlemen eagerly entered, and as precipitately retreated. One was im-

<sup>1</sup> On Fevers, Dissertation I. page 114.

<sup>2</sup> On Fevers, p. 163.

mediately seized with vomiting ; the other (the clergyman) felt sick and faint. After a little, they recruited and called the wretched woman from her prison hole, in which she had been for weeks immured. It was a small dark closet without *light* or *air*, and in it she had been immured without a change of clothes. At the end of a week both gentlemen had fever ; both took to their beds almost on the same day. The clergyman died, and the other recovered with great difficulty after a severe struggle. Both cases were alike throughout, except in the termination. The woman had not then or afterwards any febrile disease, and had been free from any at any period of her confinement.

Bursts of fever from this cause occur, at times, in situations where no possibility of contagion from without exists—as in prisons, in surgical hospitals, and in situations in which typhus does not usually prevail and has not been introduced from without. Dr. Harty gives unequivocal testimony of this fact, derived from his experience in the Dublin prisons. For cases occurring in crowded wards of hospitals during cold weather, we may refer to Palloni,<sup>1</sup> Currie,<sup>2</sup> Tweedie.<sup>3</sup>

Dr. Ferriar<sup>4</sup> gives a decisive instance of fever arising in the habitations of the poor from this cause at Carlisle in 1778—9. We must be content to refer the reader who may be desirous of sifting the evidence on this much disputed question, to the above writers, as a recital of the cases would occupy too much space. It cannot be doubted that this depraved atmosphere has been sometimes considered as a source, when it really only favoured the diffusion of the fever poison, whether emanating from the bodies of typhous patients or from paludal sources. We shall have occasion to recur to this subject when examining the circumstances which favour the diffusion of fever as an epidemic disease. At present it may be remarked that, the writers on both sides of the question, have relied in some instances, upon exceptionable proofs. Thus Dr. Peebles, in his valuable paper, adduces several cases which occurred on board ships, which are seldom free from some of the paludal sources. It is also sagaciously remarked by Lind, that it is in ships going from home, and not in those returning from the longest voyages, that fever is found. The reason is obvious.

But if weak cases have been adduced in proof of the origin of fever from this source, they have equally been relied on by the great opponent of the doctrine and his followers. Dr. Bancroft has rested much of his argument upon the fact, that on board slave ships, where the crowding was unprecedentedly great, fever was unknown.<sup>5</sup>

<sup>1</sup> Quoted by Dr. Peebles, Edin. Med. and Surg. Journ. No. 125.

<sup>2</sup> Medical Reports, page 6.

<sup>3</sup> Clinical Illustrations.

<sup>4</sup> Medical Histories, vol. i.

<sup>5</sup> On Yellow Fever, p. 127, &c. It is worthy of notice, that in the passage quoted from Dr. Lind (page 128,) the liability of felons in transports to fever is asserted.



But, as has been well observed by Dr. Fergusson,<sup>1</sup> there are two good reasons for this.

1st. The absence of all fomites—the wretches being naked, there was nothing to retain the effluvia.

2d. The high temperature, which is always destructive of the poison of typhus.

The absence of fever from the huts of Fins and Russians, may be explained in a similar way, by the high artificial heat constantly kept up in them, and the total absence of moisture. None of the advocates of exclusive contagion, from Bancroft to Davidson, add any facts to the meagre evidence upon which the argument is founded. 'The inquiry is altogether one of the most important connected with the subject of fever, and bears strongly in its consequences upon science and humanity.

For if it appear that the poison of typhus can be generated *de novo*, under the conjoint action of the above mentioned moral and physical causes, we should institute inquiries as to the part which each performs in the production of this result, and without wishing "*to get rid of a difficulty*," we should, on other grounds than our inability to trace contagion to its primordial source, pursue the investigation of its laws, disregarding any such affectation of strict logic as is contained in the following passage.<sup>2</sup> It is not intended, however, to enter into any speculations respecting the primordial source of the contagion of typhus, for the sources from which it, as well as that of the other contagious fevers originated, are involved in absolute obscurity; and though we could trace them to the most remote era in antiquity, the same difficulty would be encountered. Some authors, apparently to get rid of this difficulty, and to account for the occurrence of typhus, where no contagion could be traced, have adopted the opinion, that it may be generated by common causes, such as impure air, filth, &c. and be afterwards capable of propagation by contagion. *The argument of analogy is directly opposed to this belief, for if in nature there be no exception to the law, that two causes are never required to produce precisely the same effect, it will follow that, whatever cause can be best reconciled with the phenomena of typhus, must be considered the true source of the disease.* And accordingly this writer proceeds to return a hasty verdict of "not proven," upon the claims of every cause but this "one true source," *contagion*.

The following remark of the venerable Dr. Stokes upon this subject is too apposite to be passed over without notice. "This supposition of a *single cause* of the effects we witness, is quite unsupported by nature. Every animal, every plant, every rock, requires for its production the co-operation of many causes that we know, and most probably of many more that we have not yet discovered. All nature depends ultimately on a single cause, but it has pleased

<sup>1</sup> Edinburgh Medical and Surgical Journal, No. 112.

<sup>2</sup> Davidson, page 2.

the Almighty to cause that the effects which concern us immediately should arise from the co-operation of several of his creatures.”<sup>1</sup>

Again, if it appears that the febrile poison can be thus generated, we need not follow Dr. Barker<sup>2</sup> to the Continent of Europe to look for it. Nor need we to accompany<sup>3</sup> Dr. Lombard upon his geographico-typhoid tour in proof that the frieze coat of the Irish labourer is its depository, in which it is exported like other “native manufactures.”

But the question has a great bearing upon humanity and political economy. Take the case of an epidemic such as has prevailed in Ireland during the past year. Suppose that in a town containing a great number of poor in which fever perhaps has not yet appeared, the inhabitants meet to confer upon the best preventive measures. These will differ as their views of the sources of the disease differ; one may suppose that the contagion is in all cases *imported*, and can see no protection except in a “cordon sanitaire.”

Another believes that fever is exclusively of *endemic origin*, and he says—make sewers, sweep away the dung-hills—whitewash the houses.<sup>4</sup> While the man *alone* who conceives the generation

<sup>1</sup> Essay on Contagion. page 25.

<sup>2</sup> Dublin Medical Transactions, vol. ii.

<sup>3</sup> This notion of Dr. Lombard's, along with an opinion expressed by Mr. Farr, in the article Vital Statistics, in M'Cullagh's Statistics of the British Empire, “that the poor Irish are keeping up, if they are not introducing, the fevers of their wretched country in the heart of the British cities,” has been met by Dr. Cowan, and by an acute reviewer in the Dublin Medical Journal, for January, 1838. But the latter, while he confers a merited castigation upon Dr. Lombard, bears too hard upon Mr. Farr, whom he classes with certain humane political economists who wrote, that it would be well that Ireland were sunk in the sea. And says there is not the slightest evidence that the labouring classes *introduce* fever into the hearts of British cities. Probably not. In the case of Glasgow, Dr. Stuberoh's paper, Dublin Journal, No. 39, would seem to show that they do not—at least by *importing it*. But in an able and temperate reply in the second edition of M'Cullagh's book, Mr. Farr has shown that, in the three great avenues by which the Irish labourers enter the Kingdom—Bristol, Liverpool, and Glasgow, their crowding to excess in lodging-houses, their loathsome diet and filth, are productive of epidemic fever, and he concludes with the following wise remarks.

“In directing attention to a weighty sanitary fact, it is far from our intention to convey any reflection upon the Irish people. We shall, in treating of epidemics, show that the English were formerly in as bad a condition as the Irish, and we must say we had imagined that any attempt to prove that England is vitally interested in the prosperity and happiness of Ireland, would be rendering neither country disservice. . . . . Reduce your neighbours to ruin and starvation, and you inevitably give rise to diseases which lower like avenging angels over your own heads. . . . . So God avenges oppression; it reaps the fruits of its own handiwork.—(M'Cullagh's Statistics, vol. ii. p. 529.)

<sup>4</sup> See Sanitary Reports of Poor Law Commissioners, p. 14, and Report of the Select Committee on Health of Towns, p. 111.

Also the following passage from a Report of Dr. Addison's Essay on Malaria, Lond. Med. Gazette, vol. iii. N. S. p. 796.

“He thought that if any *palladium* could be discovered potent for the salvation of the city, it would be found in the shape of a *scavenger* !”

of the poison under the foregoing circumstances, possibly will recommend the true prophylactics, and, by providing clothing and fuel, cause the light and air to be admitted into their crowded dwellings, and by relieving mind and body from the pressure of impending starvation, will both render them less susceptible of disease *if* it approach them, and less capable of generating in themselves the poison which he believes may arise among them without exterior communication.

On this question it is impossible to speak of humanity and political economy apart. The following extract from Dr. Alison's essay on the management of the poor in Scotland, will prove how even motives of economy should lead to the application of the true preventive relief of the wants of the poor.

“‘A fever which consigns thousands to the grave,’ says Dr. Harty, ‘consigns tens of thousands to a worse fate—to hopeless poverty; for fever spares the children and cuts off the parents, leaving the wretched offspring to fill the future ranks of prostitution, mendicancy, and crime.’ ‘The mortality of fever,’ says Dr. Barker, ‘is most frequent where it is most injurious, viz. in men advanced in life, the heads and supports of families, the increase of poverty and mendicity, and the agonising mental distress to which it must give rise, are consequences which must occur to every reflecting mind.’ There is no exaggeration in the simple and impressive statement of Dr. Cowan—that ‘the prevalence of fever presents obstacles to the promotion of social improvement among the lower classes, and is productive of an amount of human misery credible only to those who have witnessed it.’ In the last situation in which I have seen fever prevailing epidemically in Edinburgh, (new land at the foot of the old fish market close,) I find, on inquiry, that five families out of the inhabitants of twelve rooms in the two upper flats of the house, have been rendered fatherless by it.” p. 9.

We could parallel these cases in this town, but it is unnecessary. There is one more consideration arising from this subject—it is a selfish one, and therefore not the least powerful—it is contained in the following profound reflection of the excellent Ferrier. “The diseases arising from wretchedness differ in this respect from those of luxury; the first are generally *infectious*, the latter solitary but *hereditary*. This observation would furnish an excellent moral, but as it is needless to suggest it, I pass on to my next point.”

SECT. IV.—*The Mode of Action of the Poison, and the Circumstances which assist its Operation in the Human Body.*—The opinions of the majority of physicians of the present day are divided, as to the *theory of fever*, into two parties—the solidists and the humoralists.

That of the former party is thus announced in the article *fever*, Library of Medicine, by Dr. Christison. “The theory of fever, then, which seems most consonant with the whole facts, with the general sentiments of the profession, especially in Britain, and with a sound and prudent practice, is probably the following. Fever is



an essential or primary disease. The first appreciable event in the chain of sequences constituting fever is a functional injury of the nervous system. The only essential or invariable consequence of this affection is functional derangement of most of the important organs of the body, but more especially of the brain, the circulating organs and fluid, the alimentary canal, and the skin. . . . . The changes which have hitherto been observed to take place in the blood and other animal fluids, are, like the local disorders, secondary and not primary. They may be the source of the phenomena remarked in the advanced stage of the disease, but they are not the source of the disease itself in the first instance."

If we turn to another recent work of high authority, we find the very reverse order of sequence is maintained. "It appears probable, if not certain, from what has been advanced, that in a certain class of fevers (typhoid) the blood is *primarily* diseased, and that certain changes in one or more organs take place as a consequence or secondary effect."

It will be seen that neither of these distinguished writers assigns the phenomena of fever exclusively to his system; and it has been well remarked, "that all febrile disturbances are disturbances of such vital actions as are the *joint* product of these two great factors of vital phenomena—for example, the primary phenomena of all fevers are—1. Disturbance in the formation of animal heat; 2. Disturbance in all the secerning functions; 3. Disturbance in the process of nutrition. But the formation of animal heat, secretion, and the nutritive process, are all dependent on the conjoint action of the nerves and bloodvessels. Either of these two systems may receive the first morbid impression, but the one soon participates in the changes of the other."

This last sentence involves the proper terms of the controverted question, for, while all must admit that the phenomena of fever *established* are due to the conjoint operation of the nervous system and the blood, the solidists maintain that it is upon the nervous system the morbid impression of contagion acts primarily; while the advocate of a modified humoral theory holds that the source and primary seat of typhus fevers, properly so called, is proved to be in the blood; and that the order of sequence is, first, a vitiation of the blood by the commixture of deleterious substances; next, in consequence of such vitiation, an alteration of the functions of the nervous system; and, lastly, the blood that supports the organs, and the nervous system that animates them, having suffered a general injury, a constant though not always appreciable modification of these organs in their function or in their texture."

The advocates of each theory construe the phenomena of the latent period in accordance with their peculiar views: thus, while the humoralist regards it as the time intervening between the ab-

<sup>1</sup> Dr. Tweedie. Art. Fever. Cyclopædia of Practical Medicine.

<sup>2</sup> Ferguson on Diseases of Women. Part 1. p. 97.

sorption of the poison and the manifestation of its effects on the great nervous centres—the advocates of the opposite theory consider that “the symptoms which characterise this period, whether they be slight, or whether they be severe, indicate a disturbance affecting primarily the nervous system.”<sup>1</sup>

Again,—“We are not of opinion that the time between exposure to contagion and the formation of the disease thereby caused, is a period of health: the nervous system was affected previous to any disorder of the circulating system.”<sup>2</sup>

From these extracts it will be seen that it is to the explication of the phenomena of the access and latent period of fever, and not to the *formed* disease, that each theory is to be applied, and its agreement with these phenomena tested.

This narrowing of the question deprives the humoralist of all support from the fact of changes detected in the blood subsequent to the latent period, since these may be owing to the changes in the nervous system; while, on the other hand, it reduces the available arguments for the nervous theory to two. That from the analogy of the morbid impression of contagion to the action of certain poisons—“such instantaneousness of action being supposed to be incompatible with the previous absorption of a poison into the circulation;” and, that deduced from the fact, that “a single mental shock often produces protracted disease, without the presence of any known source of the febrile poison.”

By thus limiting the dispute, much is given up by the humoralist; since he holds, “that the fluidity or diffuence of the blood, and the violent colour observed in typhus, is not the result of the disease, but, on the contrary, that they are the immediate effects of the specific cause of the fever;”<sup>3</sup> while, on the contrary, it is on the phenomena of the access that the very strongest arguments for the nervous theory are founded.

Passing by the many writers who have rested satisfied with stating their opinions of the origin of fever, without giving the grounds upon which they are founded, we shall examine the arguments for the nervous theory contained in Sir H. Marsh’s able paper on the Origin of Fever,<sup>4</sup> which are rested upon a number of histories of the access of the disease, which Dr. Tweedie has pronounced to “contain a body of evidence which should alone decide the question of the contagiousness of fever.”

It will be our endeavour, as advocating a humoral theory, to show that the evidence does not support the conclusions of its distinguished author. These conclusions are founded upon a supposed analogy of the morbid impression of contagion (or infection)

<sup>1</sup> Marsh. Dublin Hospital Reports, vol. iv.

<sup>2</sup> Barker and Cheyne’s Report.

<sup>3</sup> Vide Rostan’s Clinical Lectures on Typhoid Fever, in Johnson’s Review for January, 1841.

<sup>4</sup> Dublin Hospital Reports, vol. iv.

to the action of certain powerful narcotic poisons which is *supposed* to be exerted upon the nervous system immediately, and not through the circulation. "Though there can be little doubt," says he, "that prussic acid, when applied to the surface of the body, is ultimately absorbed, yet the rapidity of its action leads to the conclusion, that its first and instantaneous effect is on the nervous system." And Dr. Law, in arguing for a *mental* origin in one of his cases, in which the person was exposed to contagion before and at the time of seizure, says, "How are we to explain the mode of this individual's attack of fever? If we are to suppose it was contracted from exposure to contagion, we would avail ourselves of the argument of the toxicologist, who reasons that, from the very short period of time in which some poisons exhibit themselves in the system, these poisons affect the system through the medium of the nerves, rather than through the circuitous route of the circulation."

This theory of poisons being assumed, the analogy of the action of infection is thus stated by Sir H. Marsh. "From these facts it appears that the poison of contagion produces its effects *with the same rapidity as the narcotic poisons to which we have alluded*. Headache, debility, sickness of stomach or vomiting, are among the symptoms first perceived; these sensations, with the rapidity of an electric shock, are at the instant produced," &c.

This specious argument from analogy will be somewhat weakened by the following considerations:—

1. It is by no means proved, that any poison, however rapid, produces its effects upon the system, without being received into the general circulation, or before it can be carried to the brain through the medium of the circulation.

Müller's<sup>1</sup> conclusion upon this question is—"These experiments, as well as many others instituted by well-known physiologists, prove that, before narcotic poisons can exert their general effects on the nervous system, they must enter the circulation." And again—"The rapid effects of prussic acid can only be explained by its possessing great volatility and power of expansion by which it is enabled to diffuse itself through the blood more rapidly than that fluid circulates; to permeate the animal tissues very quickly, and in a manner independent of its distribution by means of the blood, and thus to produce the peculiar material changes in the central organ of the nervous system more rapidly in proportion as it is applied nearer to it." But even this explanation of Müller's—while it falls very far short of furnishing the desired analogy—would seem incorrect, since Mr. Blake has found that the poisonous effects of prussic acid in a large dose introduced into the stomach will not take place so long as the circulation through the vena porta is carefully interrupted. He even found that, on the effects of the poison being produced by removing for an instant the impediment to the circulation, the animal could be recovered upon

<sup>1</sup> Elements of Physiology, by Baly, vol. i. p. 246.



the circulation being again stopped, though the three drams of prussic acid were still in the stomach. Blake's conclusions from his interesting experiments are<sup>1</sup>—

1. That the time required by a substance to permeate the capillary vessels may be considered as *inappreciable*.

2. That the interval elapsing between the absorption of a substance by the capillaries and its general diffusion through the body may not exceed nine seconds.

3. That an interval always more than nine seconds elapses between the introduction of a poison into the capillaries or veins and the appearance of its first effects.

4. That if a poison be introduced into a part of the vascular system nearer the brain, its effects are produced more rapidly.

5. That the contact of a poison with a large surface of the body is not sufficient to give rise to general symptoms, as long as its general diffusion through the body is prevented.<sup>2</sup>

But secondly—the *suddenness of action of the febrile poison is generally speaking only apparent and not real*.

The infection of continued fever (says Christison) is, for the most part, by no means virulent. And again—fever is usually communicated by long exposure to the emanations from the sick, and seldom by any single short exposure, however decided. It is a common notion that single, brief, decided exposures often occasion an attack; and, in support of this notion, reference is made to cases where

<sup>1</sup> Edinburgh Medical and Surgical Journal, vol. 53.

<sup>2</sup> Sir H. Marsh states, that in some experiments performed by himself and Dr. Jacob, the poisonous effects of prussic acid were observed to commence in five seconds; there is therefore a discrepancy between his results and those of Mr. Blake, but the following experiment of the last gentleman would seem to show that even this short time would allow of the entrance of the poison into the circulation.

"A dram of the strongest liquor ammoniæ, mixed with five drams of water, was injected into the jugular vein of a dog. A glass rod which had been dipped in hydrochloric acid, was held immediately under the nostrils; four seconds after the introduction of the first drop of the solution of ammonia into the vein, it was plainly detected in the air expired from the lungs, by the white vapours that were formed upon its coming in contact with the vapour of the hydrochloric acid."

Dr. Christison's experiments on prussic acid (at page 657 of his work on poisons,) do not support Sir H. Marsh's views of the extreme rapidity of action of this poison. While at page 660 he admits that every argument but this is in favour of the theory of its action through the blood, *in which* it was detected by analysis in the case of a cat killed in a few seconds by the acid applied to the tongue.

But in the text I have neglected to notice the fact, so prejudicial to Dr. Marsh's analogy, that the blood in these cases of sudden poisoning is *fluid*. We are also told by Dr. Christison, that in cases of sudden death from the emanations from Parisian privies, the blood is found *black* and *fluid*.

A similar effect is observed in cases of sudden death from other kinds of miasm—for an instance from animal putrefaction, see the Medico-Chirurgical Review, for January, 1825; and for an instance from marsh miasm, see Evans on the Endemic Fevers of the West Indies, p. 22.

individuals can trace the infection, as they imagine, to a particular fever patient, by having experienced some very peculiar morbid sensation at the time of exposure. There is much room for fallacy, however, in observations of this kind, and besides their proportion is small compared with the far more numerous instances where no such sensations can be recalled as having ever been experienced.

It is unnecessary, though it would be most easy, to multiply testimony to the same effect. Even Dr. Marsh says very truly, that "by far the greater number of patients labouring under contagious fever, are not at all aware of the circumstances connected with the origin of their complaint; the impression made at the time of their exposure being in general unheeded or forgotten. Indeed the impression is often times so slight, as to lead one to think that contagion does no more than predispose to fever, and determine the nature of the disease, of which, exposure to cold, fatigue, or some such accidental circumstance, is the immediately exciting cause; so that there appears much reason to believe that, many are so mildly affected, that were it not for the superaddition of an exciting cause, they would altogether escape fever; hence it happens that numbers affected with contagious fever, trace the origin of their complaint exclusively to cold, wet and other exciting causes of the disease, the time and circumstances of exposure to contagion having been entirely forgotten. Cases of this kind, *which are by far the most numerous*, throw but little light on the origin of fever. It is only by a careful observation of facts of occasional and rare occurrence, such as those recorded in this paper, in which the effects of contagion are well marked and striking, that we can hope to obtain certain and satisfactory results."

There is much truth in the foregoing passage, especially in that part of it which assigns to contagion the action of a *predisposing cause*; but how can this view be reconciled with Dr. Marsh's own theory, that the action of contagion is an "injurious impression upon the sentient extremities of the nerves?" and how far is he justified in assigning the *cause* and *commencement* of fever to sudden and brief exposure, even by cases of rare occurrence, (exceptions he admits to the general rule), such as he has collected? These are questions deserving consideration. We shall return to the first, when examining the argument for the humoral theory, derived from the latency and cumulative property of the poison; but how do Dr. Marsh's cases support his opinions as to sudden exposure being the *cause* of fever? It is obvious that when it is committed, that the general rule is, "that no perceptible impression is made by contagion;" we cannot admit the conclusion that the impression was the cause of the disease, except it appears that no other exposure took place; the more so, since the medical and other attendants of fever patients in private houses, and where cleanliness and ventilation are properly observed, frequently perceive these impressions—arising from the odour of the patient or his excretions;—such impressions, however *sickening* at the time,

seldom leading to any further ill consequences; but of twenty-two cases adduced by Dr. Marsh, ten were nurses or porters of fever hospitals, seven were physicians, one a clergyman, and one appears merely to have suffered the nervous shock, as fever did not follow.

The remaining three appear to be unexceptionable instances of fever, arising from a single and concentrated dose of the poison; two, if not all of them, being cases of communication by fomites, (usually containing a concentrated poison.)

But again, we have to inquire whether the moment of exposure was that of the *commencement* of the fever? since the argument rests mainly on "such instantaneousness of action of the poison as is incompatible with the idea of absorption into the blood." Here we might remark on the rapid diffusion of gaseous poisons through the blood, and appeal to Mr. Blake's experiments in proof that the poison may enter the circulation even before the impression is felt; but admitting that this impression is a purely nervous one—a shock, or "reaction," as it has been termed—"a resistance offered by the vital powers to chemical action"—it is *not* the commencement of fever. For it may end where it began; the impression may not, and very often is not followed by fever; and in many more cases goes off altogether for a longer or shorter period before fever commences. True, it may continue, especially in persons whose imagination has become alarmed—in which case some writers have attributed the imagination to the influence of the poison upon the nervous system—and, in a manner hereafter to be explained, it may shorten considerably the latent period; but we repeat, this latent period will be found to exist in any case in which a *previous* imbibition of infection is not to be admitted. "The symptom," says Sir H. Marsh, "which is generally considered to mark the commencement of a febrile movement in the system, is that commotion of the nervous functions which has been technically termed a *rigor*." This commencement of the *febrile movement* is only mentioned in twelve of his cases, and in these it occurred in four at an interval of from one to two days, in six after several hours, and in two only it is said to have come on "a short time after" exposure to the poison.

The third consideration which may be urged against this analogy is, "that the poison with which contagion is compared is not *reproduced*." As this reproduction of contagion is one of the strongest arguments for the humoral theory, we shall not dwell upon it here, but merely observe that the toxicological argument, while it sets up a forced and false analogy with poisons which are not reproduced, strives to weaken and destroy that which naturally exists between the infection of typhus and that class of morbid poisons to which it may be said to belong—the exanthemata. This has not escaped Dr. Marsh's observation, who admits that "the opinion that to maintain a protracted fever, an internal cause of disease (such as absorbed or generated morbid matter) is necessary, would arise from the phenomena which manifest themselves in the course of



an exanthematous fever." But he meets this by the second of the objections we have enumerated to the humoral theory.

"Yet that to excite and maintain continued fever, an *abiding* cause is *not* necessary, might be proved in various ways, but the fact that a *single* mental shock often produces protracted disease, is decisive upon this point."

As Sir H. Marsh adduces no fact in support of the above assertion, turn we to another able physician who, in a recent paper, adduces seven cases from his own experience, in proof of the opinions expressed in the following passages:<sup>1</sup>—

"We quite agree in the wisdom of the precaution of satisfying the absorbents, but deny that they are more the channels through which the morbid matter enters the system, in this instance, (fever from contagion,) than they are in other cases where there is no reason to suppose either that they are in an unusual state of activity, nor if they were, can we discover any contagion to serve as a *materies morbi* for them to exercise themselves upon. These are cases in which a strong moral impression acts as a direct and immediate cause in the production of a fever, similar in all respects to one from contagion," &c.

And again:—

"We shall proceed to detail some cases of fever which seem to us calculated to throw some light upon the mode in which the first morbid impression is made upon the system in the production of the disease; and see how far these cases tend to confirm the opinion that fever is the result of a miasma conveyed to the system by the absorbents: *or* if it be not, in some cases at least, the effect of a *moral impression* acting upon the nervous system, and exhibiting itself in symptoms indicating a derangement of the functions of this system."

The advocate for the theory of absorption may reasonably require that in such cases the *materies morbi* shall not appear to have been within reach. But of five cases, the subjects were exposed to infection at or before the seizure. The sixth was not (as Dr. Law admits) a case of fever; and we have only one in which fever followed a mental shock, without evidence of infection at the same time existing. To explain away this case, a determined opponent of the nervous theory might adduce evidence of the general diffusion of the fever-poison through the atmosphere of a city, when fever is prevalent in it; he might maintain that at such times<sup>2</sup> "certain changes take place in the constitution of the atmosphere imperceptible to our senses, and eluding chemical tests, which predispose human bodies to febrile diseases in such a way, that circumstances which in ordinary times would only give rise to a catarrh, an attack of rheumatism, or even occasion no indispo-

<sup>1</sup> Observations on Fever, by Dr. Law. Dublin Med. Jour., vol. xiv.

<sup>2</sup> Prichard on the Epidemic Fever of Bristol.

sition at all, will now in many individuals become the exciting causes of continued fever."

If it be said that this is begging the question, the humoralist takes higher ground, and asserts that such cases, instead of disproving, strengthen his own theory; inasmuch as he can show that fever follows strong nervous impressions, in consequence of their lowering the vitality of the blood, and so favouring the transformations in that fluid upon which fever depends. He believes that<sup>1</sup> "no other component part of the organism can be compared to the blood in respect of the feeble resistance which it offers to exterior influences. The blood is not an organ which is formed, but an organ in the act of formation; indeed, it is the sum of all the organs which are being formed. The chemical force and the vital principle hold each other in such perfect equilibrium, that every disturbance, however trifling, or from whatever cause it may proceed, effects a change in the blood. Every chemical action propagates itself through the mass of the blood; for example, the active chemical condition of the constituents of a body undergoing decomposition, fermentation, putrefaction or decay, disturbs the equilibrium between the chemical force and the vital principle in the circulating fluid: the former obtains the preponderance. Numerous modifications in the composition and condition of the compounds produced from the elements of the blood, result from the conflict of the vital force with the chemical affinity in their incessant endeavour to overcome one another."

He admits that<sup>2</sup> "perhaps there are cases in which the modification of the blood is only secondary to a modification of the nervous system. If, for instance, under the influence of a strong mental emotion, this system, being suddenly perverted in its action, ceases to exert its proper influence over the different organs in which the blood is elaborated, deposited, and receives new materials, must not that fluid itself become altered in its turn? If so, thence must arise a number of organic and functional derangements varying greatly, according to the mode and intensity of the primitive alteration of the innervation. In such cases we may observe to occur sporadically those same diseases, typhoid or other, that we have just now seen prevailing epidemically under the influence of manifest causes of infection of the blood."

To prove that Dr. Law's case belongs to this formula, let us place it by the side of another in which *precisely* the same mental impression, acting more intensely, produced death. Eliza J—, æt. twenty-six, was admitted under Dr. Law's care, March 28, 1836. She had been in perfect health a week since, when, on missing a piece of linen which had been committed to her care to make shirts, from the apprehension that her honesty would be called in question,

<sup>1</sup> Liebig, p. 360.

<sup>2</sup> Andral, Pathological Anatomy, vol. i. p. 671.

she was seized with a violent rigor and sickness, which confined her to bed ever since. Petechial fever, with prominent hysterical symptoms, followed. She recovered with difficulty and slowly.

Some time ago, I was present at the examination (*post mortem*) of a man who died suddenly under the following circumstances.

He had committed a very trifling theft, for which he was apprehended and carried before a magistrate. He was a person rather above the lower order, and manifested great shame and grief at this exposure. While sitting before a table waiting for his case to be called on, he leaned his head forward on the table, and was observed to snore; in a few minutes, the sound of his breathing ceased, and on raising his head, those near him found that he was dead. It was supposed that apoplexy was the cause of death, and the brain was first examined. It was, however, perfectly healthy. The other viscera were then carefully examined. The only one which discovered any thing which could account for his sudden death, was the heart, which was distended with *dark fluid blood*.

Let us suppose that the mental impression had not been so intense in this case, and the life of the blood not so completely and suddenly destroyed—what would have been the probable consequence? This question is answered by a comparison of the two histories. In the last, the vitality of the whole circulating mass was destroyed, and the symptoms were those of a brain suffering the influence of *a strong narcotic poison*. In the other, the livid, petechiæ, spongy, and bleeding gums, &c., showed to what an extent the vitality of the blood had been destroyed. The immediate occurrence of a rigor showed that the self-generated poison had reached the nervous centres, and that the struggle had commenced which was to end with either the death of the whole mass of blood, or the elimination from it of the portion so affected. It is worthy of remark, (and is noticed by Dr. Law,) that the rigor was immediate—not after an interval of hours or days, as in cases of exposure to infection, in which the operation of the poison is gradual and often (generally, indeed,) accumulative.

In fine, typhus, or a disease resembling it, but differing, according to Dr. Cheyne, in the very important particular that it is not communicated by contagion—in other words, that the poison is not *re-produced*—is but one of three modes, or degrees, in which the blood suffers from a strong mental impression. It may be killed at once, or it may suffer in a degree insufficient to produce *formed disease*—loss of appetite and depraved secretions, with slight derangements of animal heat, being perhaps the only indications of the injury it has received—or it may act upon the system in a manner similar to the fever poison. But this cannot be said to prove that the fever poison acts by producing a moral impression; and, therefore, instead of agreeing with Dr. Law, that “even in cases where there was most reason to suspect absorption, where a person having exposed himself to contagion, fasting—and then contracted the disease—even here the symptoms exhibited by the disease so resemble



those where there is no possibility of suspecting infection, that we cannot but believe that the mode of absorption is the same in both cases, and that as it is not absorption in the one case, neither is it in the other"—instead of going to this length of denying the existence of a *materies morbi* altogether, we would reduce the two cases to the same formula by an opposite method. As thus: violent nervous shocks kill the blood or modify it, and occasionally produce fever. Contagious and other miasms also, in some rare instances, kill the blood, and, in general, modify it, so as to produce fever. But they may do so without causing a nervous shock. Therefore, they act *directly* on the blood, by being absorbed into that fluid, and not through the intervention of any derangement, functional or otherwise, of the nervous system.

The principal arguments for the nervous theory derived from the mode of access of fever, having been examined, we shall submit some of those which tend to support a modified humoral theory, and then offer a rationale of the action of the *causes* of fever in accordance with this theory.

The explication of the accession of the disease having been taken as a text of the opposite theories, we are deprived of any support from two arguments which have been much used by humoralists: viz. the changes which the blood undergoes in the course of fever, and the production of fever or a disease perfectly analogous, by the introduction of substances into the circulation.

Another argument, of a similar kind, is derived from the known power of sources which ordinarily produce fever, to kill the blood at once when their poison is introduced into it in sufficient quantity. We give the fact on the highest authority.<sup>1</sup> The inference has been met by the toxicological argument already considered, and by a distinction asserted between nephritic poison and the fever poison. This distinction we shall examine, along with the source itself, hereafter.

But there are certain peculiarities in the action of the febrile poison which in their general character resemble other morbid poisons, and favour the idea of its absorption into the blood.

The first of these is its occasional latency in the system, in which it will lurk for a longer or shorter period, until called into action by some accidental cause.

"In several instances," says Dr. Graves,<sup>2</sup> "I have observed that certain diseases, which seemed to have been lurking in the constitution, may suddenly make their appearance in consequence of the operation of causes apparently unconnected with the disease in question. . . . I have witnessed several bad cases of bad secondary venereal, in which the attack was traced to excessive fatigue, or a common cold. You will also meet numerous examples of an analogous fact among fever patients: examine them, and you will

<sup>1</sup> Christison on Poisons, p. 700, 2d edition.

<sup>2</sup> Lectures, London Medical Gazette, vol. iii. N. S. p. 186.

learn that in a majority of cases their disease arose from exposure to cold. One person fatigues himself by too much exertion in business, and gets an attack of spotted fever; another attributes his disease to over anxiety; some to intemperance, and some to fright. In all these cases, it is very probable that the poison of fever has been lurking for some time in the system, and has been called into active existence by the operation of some sudden accidental cause, as fright, fatigue, intemperance, or cold."

Something similar, Dr. Graves justly observes, is remarked in the case of the Irish labourers employed during summer and autumn among the fens of Lincolnshire (and we may add Cambridgeshire.) During their stay in England, they appear free from disease; but on their return home, if they happen to be exposed to wet, fatigue, or the derangements of health consequent on intemperance, they are very often seized with intermittent fever.

He continues, "Does it not often happen, that many of us escape fever although exposed to its contagion month after month? Do we not go on for years untouched, although subject every-day to the imbibition of the poison? and do we not, rendered bold by our impunity, consider ourselves, as it were, fever proof, until some accidental cause convinces us of the contrary, by giving rise to a sudden and violent attack? Who is there that has not observed this repeatedly among the students attending a fever hospital?"

Similar proof of the latency of the fever poison is afforded by the cases recorded by Lind, of sailors, who apparently escaping from the fever which was raging on board, went ashore, and in some time afterwards, in consequence, apparently, of exposure to cold or debauchery, were attacked, *not* with the fever prevailing there, but with that of the ship they had left. In this respect, then, the febrile poison resembles other morbid poisons.

Again: *it is a cumulative poison.* The exposure of a single moment is probably insufficient, in any case, to cause fever. A few inspirations may accumulate sufficient in cases of great concentration of poison; but there is abundant proof that daily and continued imbibition of the poison is, in general, requisite. Thus, we find the attendants on the sick attacked in proportion to the frequency of their approaches to the infection, the very reverse of what would be the fact if the poison were *not* cumulative, since it is a law constantly observed, that agents which act by single impressions lose their power of producing those impressions in proportion as they are frequently repeated. It is true that some eminent writers aver this of infection, as Dr. Copland, who says, "when a person has escaped infection upon the first or the earlier exposures to several infectious maladies, he will generally continue to possess an immunity, unless circumstances should occur to increase his predisposition." Observations made on a large scale, however, tend to disprove this, as regards typhus.

Thus, when fever prevailed during the retreat of the British

army through Holland, we are informed by Dr. Fergusson,<sup>1</sup> that few, indeed, of the medical staff escaped the typhoid contagion; and, again, in the retreat from Talavera to the confines of Portugal, it was seen that the *best seasoned* of the medical staff were the principal sufferers. Dr. Christison, too,<sup>2</sup> (a solidist) maintains that it is not improbable that the severity of the disease bears some proportion to the *amount* of exposure.

And . . . . . "In many instances, fever breaks forth apparently from gradual charging of the constitution under *constant exposure* to the morbid emanations, and without any other co-operating cause."

'This is very like humoralism, as is the illustration given by Dr. Haygarth. "A pint of yeast will excite fermentation in a barrel of ale, but a hundredth or a thousandth part would not have the same effect."

Again. The *reproduction of the poison of contagion*, is a fact "not dreamt of" in the philosophy of the solidists. Here their analogy is at fault, for the poisons from whose action it is derived are not reproduced. Neither will any supposable impression upon the nervous system explain the continued reproduction of the same febrile phenomena, and the same miasm through an indefinite series of individuals. We have admitted the production of fever by a strong mental impression. We have endeavoured to reconcile this occurrence with the theory which refers the source of fever in all cases to the blood. We have, however, noticed the fact, that such fever does not reproduce itself, and referred to the testimony of one, whose accuracy of observation has seldom been surpassed, who says, "The most remarkable part of the disease is that it does not spread. I have no recollection of a second case of this kind of fever occurring in a family."<sup>3</sup>

But the humoral theory has its analogy for the reproduction of the poison.<sup>4</sup> "The mode of action of a morbid virus, exhibits such a strong similarity to the action of yeast upon liquids containing sugar and gluten, that the two processes have been long since compared to one another, although merely for the purpose of illustration. But when the phenomena attending the action of each respectively, are considered more closely, it will in reality be seen that their influence depends on the same cause."

Now, when yeast is introduced into a mixed liquid, containing both sugar and gluten, such as wort, the act of decomposition of the sugar effects a change in the form and nature of the gluten, which is in consequence also subjected to transformation. As long as some of the fermenting sugar remains, gluten continues to be separated as yeast, and this new matter, in its turn, excites fermenta-

<sup>1</sup> Edinb. Med. and Surg. Journal, No. 112.

<sup>2</sup> Library of Medicine, Art. Fever.

<sup>3</sup> Dr. Cheyne's Account of Fever from Mental Causes, in Sir H. Marsh's Paper on the Origin of Fever.

<sup>4</sup> Liebig.



tion in a fresh solution of sugar or wort. If the sugar, however, should be first decomposed, the gluten which remains in solution, is not converted into yeast. We see, therefore, that the *reproduction of the exciting body* here depends:—

1. Upon the presence of that substance from which it was originally formed.

2. Upon the presence of a compound, which is capable of being decomposed by contact with the exciting body.

If we express, in the same terms, the reproduction of contagious matter in contagious diseases, since it is quite certain that they must have their origin in the blood, we must admit that the blood of a healthy individual contains substances, by the decomposition of which the exciting body or contagion can be reproduced. It must further be admitted, when contagion results, that the blood contains a second constituent, capable of being decomposed by the exciting body. It is only in consequence of the conversion of the second constituent, that the original exciting body can be reproduced.

When a quantity, however small, of contagious matter, that is of the exciting body, is introduced into the blood of a healthy individual, it will be again generated in the blood just as yeast is reproduced from wort. Its condition of transformation will be communicated to a constituent of the blood; and in consequence of the transformation suffered by this substance, a body identical with or similar to the exciting or contagious matter, will be produced from another constituent substance of the blood. The quantity of the exciting body newly produced, must constantly augment, if its further transformation or decomposition proceeds more slowly than that of the compound in the blood, the decomposition of which it effects."

These substances are the organic matters existing in the blood, either in the state of transition from blood into the constituents of the tissues, or from food into blood. Which changes, it is argued, cannot take place without the formation in the blood of new compounds, which require to be removed by the organs of excretion.

"When the organs of secretion are in proper action, these substances will be removed from the system; but when the functions of these organs are impeded, they will remain in the blood, or become accumulated in different parts of the body. The skin, lungs, and other organs, assume the functions of the diseased secreting organs, and the accumulated substances are eliminated by them. *If when thus exhaled, they happen to be in the state of progressive transformation, these substances are contagious, that is, they are able to produce the same state of disease in another healthy organism, provided the latter organism is susceptible of their action; or in other words, contains a matter capable of suffering the same process of decomposition.*

"In the abstract chemical sense, reproduction of a contagion

depends upon the presence of two substances, one of which becomes completely decomposed, but communicates its own state of transformation to the second. The second substance thus thrown into a state of transformation, is the newly formed contagion.

"The second substance must have been originally a constituent of the blood; the first may be a body accidentally present.

"If *both* be constituents indispensable for the support of the vital functions of certain principal organs, death is the consequence of their transformation. But if the absence of the *one* substance, which was a constituent of the blood, do not cause an immediate cessation of the functions of the most important organs, if they continue in their action, although in an abnormal condition, convalescence ensues. In this case, the products of the transformations still existing in the blood, are used for assimilation, and at this period, secretions of a peculiar nature are produced."

Having submitted this chemical analogy of the reproduction of contagion in the words of the highest living authority on animal chemistry, it only remains to attempt a rationale of the action of the causes of fever, in accordance with its principles, which may be thus stated:—1st. That the principal character of the blood consists in its component parts being subject to every attraction; the chemical forces of this fluid, and the vital principle holding each other in such perfect equilibrium, that every disturbance, however trifling, or from whatever cause it may proceed, effects a change in the blood.

2d. That bodies, the elements of which are in a state of decomposition, or transposition, when produced from the blood, as contagions are, will communicate *their state* to the sound blood, exactly as gluten in a state of decay or putrefaction, (yeast) causes a similar transformation in a solution of sugar and gluten (wort.)

Assuming then, that the primary action of the febrile poison is upon the blood, there can be but one *essential cause* of fever, viz., *The introduction of the poison into that fluid.* Its activity, or the occurrence of the peculiar transformations which it has a tendency to excite in the blood, will be determined by the existence of certain accessory or accidental causes, which disturb the equilibrium between the chemical forces in the blood and the vital influence; either by their action on the blood, causing the increase of compounds subject to those transformations which the poison produces—as depraved diet, bad air, &c.; or by their action on the nervous system, withdrawing permanently or temporarily more or less of its influence, and so favouring the chemical action of the poison. Such are the depressing effects of cold, fatigue, anxiety, debauchery, disgust, fear, &c. These are usually termed *exciting causes*, the former *predisposing causes*.

The occurrence of fever—the length of the interval which may elapse between the imbibition of the poison, and the first febrile movement; in other words, the length of the latent period—the severity of the disease, and the facility with which infection is

received and communicated, will depend upon the relative power of the poison, and its combination with one or more of the foregoing predisposing and exciting causes.

Thus, the continued imbibition of the poison will sometimes, apparently without the co-operation of any accessory cause, result in an attack of fever. This, however, is a very rare case, as though deranged health, and particularly disorder of the receiving functions, may exist, the poison is in this case usually eliminated from the blood, unless the balance of forces in that fluid be disturbed by some one or other of the exciting causes.

The occurrence of the exciting cause may be, or may not be, accompanied by exposure to contagion. In the case of nurses, and the other attendants of the sick, some single exposure being marked by the presence of an exciting cause, it has been supposed that the infection was then and there received into the system, when, in reality, it was before latent, and only rendered active by the *circumstances accompanying* this particular exposure. Again, when the occurrence of the exciting cause is not attended with exposure to infection, the fever is often wrongly attributed to cold, excess at table, mental emotion, &c., the latent presence of the predisposing contagion not being recognised by the patient, and sometimes, as we have seen, being denied by the physician.

The exciting cause may act, not only by determining the occurrence of fever, but also by shortening its latent period.

This is a frequent effect of exposure to infection. "In these cases, the ascertained laws of incubation," says Fergusson, "will so far be set at nought, that a terrified patient will not only fix the precise moment of infection, but will actually sicken prematurely with small-pox, (a latent infection must of course have been previously received,) through the spectacle of the disease in the person of another, or through the disgust (and nothing worse) of an excremental smell, strongly affecting his alarmed imagination, or through the same impression, he may fall down the victim of an impossible contagion, like that of yellow fever."

The apparent shortening of the latent period of morbid poisons, seems to occur under these circumstances:—

1. A strong impression made on the nervous system at the time of exposure. If this be so powerful as to affect seriously the vital principle, the effects of the poison will follow with proportionate rapidity. The poison of ague, usually so long latent, affords a good illustration:—Dr. G. Bird relates, "that being employed in some experiments upon the gas in marshes (near Woolwich,) having suddenly disengaged a quantity of most offensive gas, he was seized with nausea; and on the day following, with intermittent fever."

A similar instance, in his own person, is related by Mr. Evans; and another, in which death followed in forty-eight hours. *In this last case, the blood was found fluid.*<sup>1</sup>

<sup>1</sup> On the Endemical Fevers of the West Indies.



2. A less powerful impression upon the nervous system may accompany exposure, and be followed by a latent period, apparently shortened, but admitting of the supposition of infection previously latent. Several of Sir H. Marsh's cases afford illustrations of this fact. And it is very probable that exposure to contagion in this way often produces merely the same effect, as an exciting cause, that cold or any depressing agent would exert.

3. The circumstances accompanying exposure to one kind of poison, instead of acting as accessories to the action of that poison, may cause the immediate action of another, previously latent.

This is the only reasonable mode of explaining the cases of irregular contagion, related by Marsh and others, of typhus, received from small-pox patients, scarlatina from typhus, *ague* from typhus, and typhus from puerperal fever.

Some of these cases we might truly term *impossible* contagion, unless explained by the supposition of a previously latent poison. The facility of reception of the disease depends upon two conditions; 1st, the presence in the blood of compounds capable of undergoing the transformation of the poison. This constitutes susceptibility; and when it exists in a great degree, and conjoined with, 2d, diminution of the vital influence, it constitutes the highest degree of predisposition to disease. The proneness which the living body may thus acquire to infection, may be so great (as seen in crowded collections of wretched beings in large cities, deprived of air, light, fuel, clothing, and sustenance,) as to resemble that incapacity of resisting the progress of decay, (a true contagion) which is exhibited by dead animal matter, placed in a putrefying atmosphere.<sup>1</sup>

The severity of disease depends partly on the above circumstances, but principally upon the *dose* of the poison. This may be illustrated by comparing small-pox and measles, received in the natural mode, with the same diseases communicated by inoculation. Individuals may suffer as severely from the latter as the former, but the generality of persons do not. The following passage in a recent work of great ability offers inducements to consider this subject somewhat in detail:—"The modifications in disease dependent *on the mode of introduction* of the morbid cause, is, however, a subject too difficult for me to grapple with, and the observations are too few to offer any precise result. Cruveilhier, in the article '*Phelibitis Dict. de Med. et Chir. Prac.*' points out the increased intensity of effect when pus is introduced into the circulation at once, and as compared with that caused by gradual absorption from an abscess. The modification which small-pox undergoes by inoculation, as compared with that malady acquired by inhalation, is very remarkable."<sup>2</sup> From this last observation it would appear that the author considers the modification of small-pox as not *con-*

<sup>1</sup> A fact noticed by Parent Duchatelet, in some infectious places in Paris; and by Senac, see Wilson Philip on Fevers, vol. i. page 210.

<sup>2</sup> Fergusson on Diseases of Women, page 104.

*sistent* with Cruveilhier's observation. Such an idea must have arisen from confounding the *matter* of the small-pox pustule with the *poison* of small-pox,<sup>1</sup> when in reality it only contains the poison in common with the blood and all its excretions.

<sup>1</sup> The distinction between them is well stated by a writer in the 'Edinburgh Medical and Surgical Journal,' vol. liii. p. 206.

"Rayer mentions 'pus and miasm' as two distinct agents which should never be confounded. If the contagious effluvium and the matter of the pustule were one and the same thing, how could we account for the circumstance of the *fœtus in utero* becoming affected with the small-pox? Besides, Dr. Waterhouse and others have recorded cases in which persons exposed only to the exhalations from the blood of small-pox patients have been afterwards attacked by the disease."

The fact marked in italics would also serve to prove the distinctness of the poison from the ponderable matter of lues. Other considerations would lead us to extend it to all morbid poisons. For

1. The peculiar action of a morbid poison on the blood presumes its possessing great diffusibility in that fluid; and this quality is known to exist in all substances universally, as the cohesion of their atoms, or in other words, their ponderability.

2. The power of permeating tissue depends upon the same condition; and while we find that all the morbid poisons *may* act without abrasion of surface, we find that those which do appear to permeate the skin, act with more certainty if presented at a temperature which admits of their volatilisation. This is notoriously true of small-pox, as the dissection of subjects who have died of this disease, though not harmless, is much less infectious than the handling of the living body. It is also well known that the examination of any dead body is most likely to be followed by the bad consequences of a dissecting wound, when the body is warm and contains the halitus of its cavities uncondensed. . . . . the next in point of danger being that which is next in diffusibility; the exposure of the surface of the hands to the liquid contents of the serous cavities in particular cases, especially in puerperal peritonitis.

I need only refer to Mr. Stafford's paper on this subject in the 20th vol. of the "Medico-Chirurgical Transactions" for instances of the imbibition of this poison in puerperal and other cases, without any abrasion of surface. The following circumstance bearing on this subject occurred to myself a short time since.

I was sent for to see a lady in the latter end of her first pregnancy, who I was informed had been for some time suffering much painful anxiety of mind and fatigue of body, and had been laboriously occupied with the arrangements for entering on a new residence, which had kept her constantly upon her feet. For some weeks the legs had swelled considerably, and pitted under pressure. This swelling had rather suddenly increased, and extended to the thighs and pelvic region, with a feeling of stiffness and inability to walk up stairs. Her pulse was quiet, tongue clean, and general health apparently perfect. This was on the morning of the 21st of December. All appeared to go on well, and the swelling seemed to diminish a little till the night of the 23d, when she slept none, and was attacked with vomiting. On the morning of the 24th I found her remarkably changed; the countenance haggard and anxious, with a quick irritable pulse, thickly furred tongue, restlessness, and vomiting of a dark green fluid. Labour pains came on at 10 a. m. and continued regularly during the day. About 10 p. m. she had an attack of convulsions, and in a few minutes another. Delivery was immediately effected by the assistance of the forceps. It was observed that the labia had since morning become very dark coloured, and the perineum tore upon the slightest stretching like wet brown paper, but *without bleeding*.

The poison of small-pox is equally subtle and imponderable with the other morbid poisons, *an aura*, present it is true in the matter of the pustule, but equally present and equally capable of communicating the disease in the gaseous exhalation which arises from

The delivery of the child was followed by that of a second, unassisted; both being quite dead and flaccid. The uterus contracted firmly, and there was no hemorrhage; but the patient became less and less capable of being roused, the abdomen enormously distended, respiration laborious, and she sunk at 2 a. m. three hours after delivery.

About four in the evening of that day, I felt a hot painful itching upon the back of my right hand, where I perceived a small transparent vesicle. In a couple of hours I had pain in the axilla, and an uncomfortable, chilly feel. I applied a number of leeches to the hand and took an emetic, followed by calomel and James's powder. These means removed all unpleasant general symptoms, but the part itself did not recover so speedily, as an ill-conditioned obstinate sore formed on the hand which was long in healing. Not the slightest scratch or puncture existed before the application of the poison.

But whence this poison? It was ingeniously suggested to me by my friend, Dr. Clifford, who assisted me through this most distressing case, that the vital powers being over-taxed for the nutriment of two children, had given way, and this decomposition before death was the consequence. Perhaps this is the only explanation which can be admitted.

But I am inclined to believe that the whole was the effect of phlebitis, by which a morbid poison was generated, which produced *death in the fetuses*, disorganisation in the mother, and being presented under circumstances favourable to absorption, rapidly permeated the skin to which it was (only for a few moments) applied. Every thing was favourable to the occurrence of crural phlebitis and to the absorption of the poison into the patient's system, as will appear from the history of the case, without again enumerating particulars. Dr. Wilson's paper, in the "London Medical Gazette" for April 1838, proves that crural phlebitis in women is not confined to the puerperal state.

*Note.*—It was not till after the section upon the Theory of Fever had been sent to press, that I met with Dr. Hodgkin's remarks upon the nature of the fever in his recently published volume on diseases of the mucous membranes, Lecture 23d. "I shall now proceed to state what I have conceived to be the condition of the system which constitutes fever, whether it be produced by the influence of some local inflammation or lesion, or exist by itself, independently of such exciting cause. This latter form, however, if it have an existence, I regard as of much rarer occurrence than has generally been supposed. *Fever, I imagine, to depend on the suspension, or at least very considerable interruption of that process by which, during health, the various parts of the system are continually undergoing a change, the old materials being removed, whilst others are substituted in their place. . . . .* The process of incessant and universal change of the particles constituting our frames is what we imply by the terms nutrition and 'interstitial absorption,' it is not merely in its character closely allied to secretion, they are, I believe, essentially parts of the same function," &c. This view is supported by strong facts derived from the phenomena of fever, and by much ingenuity of reasoning. And Dr. Hodgkin proves, *at least*, that such an arrest of the molecular change takes place with reference to the secretions and nutrition of the body in fever. Thus far, there is a coincidence between his theory and that advocated in the foregoing section; the same suspension of secretion and accumulation of organic matters in the system, being part of both explanations, and the phenomena of solution or crisis being explained similarly in both.



the blood drawn from a variolous patient. The same is true of measles, which has been propagated over and over again by Home, and others, by inoculation with the *blood* of the patient, and with the same result,—a milder form of disease. By thus separating the poison from its vehicle, the difficulty of explaining the modification of these diseases by inoculation is got rid of, since, to recur to the simile of Dr. Haygarth, a hundredth part of a pint of yeast will not excite fermentation in a barrel of ale, though a pint will do it; and it must be obvious that a single inspiration in the immediate neighbourhood of a small-pox patient may introduce more of the *aura* into the blood than the direct introduction by inoculation of an atom of matter, in which only a small proportion of the poison can be present. The correctness of this view could be readily tested; and if it were found that, as in typhus, the amount of exposure had an influence in determining the severity of the attack of small-pox, the explanation must be admitted. One fact is strongly presumptive in its favour; it is the less complete removal of the susceptibility to the disease after inoculation than after natural small-pox. The analogy to the fermenting process is too obvious to need suggestion, and the same remark holds good of fever, short and mild fevers being notoriously more prone to recurrence than a long and severe form of the disease.

SECT. V.—*The Characters of the Disease produced by the Infectious Animal poison of Typhus.*—The argument for the foregoing theory of fever, would obviously be much strengthened if it could be made to appear that the phenomena of Typhus are so analogous to those of the other morbid poisons, as to entitle it to a place among “those special contagions, which do not amount to more than five or six, and are all comprehended under that class of which it is the general distinguishing characteristic to occur once only during the life-time of the individual;” in other words, to be classed with the exanthemata.

The difference is as to the *initiatory movement*. While Dr. Hodgkin would consider the *factor* of the disease to be in all cases a local lesion or inflammation, the theory of a morbid poison supposes it to be a *molecular change in the blood* caused by the dynamic force of the decomposing particles of the poison, from which arise disturbance of the process of innervation, and of the molecular changes of nutrition, interstitial absorption, and secretion.

The theory does not assume to *determine* whether the changes in innervation (such as rigor) which mark the commencement of *formed* fever, are the direct effects of the poison carried through the circulation to the nervous centres, or whether, as Dr. Hodgkin infers, from a conversion of Edwards’s proposition, “since cold has the effect of retarding, especially that function by which particles to be rejected from the body are thrown off, a *suspension of this process from another cause should be attended with a sensation resembling in a degree those caused by cold.*” This seems rather a doubtful conversion of Dr. Edwards’s fact. I shall hereafter return to Dr. Hodgkin’s ingenious speculations, merely observing for the present, that while some parts (see page 491) support a humoral theory, his theory will by no means explain the phenomena of *infection* as the humoral theory does.

We find medical writers much divided upon the question whether the petechial eruption of typhus is a primary and essential, or a secondary and accidental character of the disease. We may refer to De Haen,<sup>1</sup> Hoffman, and especially to Bruserin's elaborate argument for the former opinion, and for the consequent classification of typhus among the exanthemata; and among more recent writers the same view is ably supported by Dr. Copeland and Dr. Peebles, Dr. Roupell, and Dr. Davidson. Dr. Alison seems inclined to adopt it, though his language is reserved and cautious. "Such cases of spotted fever may be said to form the link that connects the order of fevers with that of the contagious exanthemata."<sup>2</sup>

If it be found that the analogy is complete in every essential particular, and that the objections which have been urged against the classification of typhus with the exanthemata are founded upon supposed discrepancies, which have no real existence, we shall be entitled to substitute for this cautious approximation, the decided definition of Dr. Peebles: "This contagious febrile eruption is an exanthematous affection, the production of human effluvia where society is placed in circumstances favourable to its developement, and should be considered the effect of a poison *sui generis*. It arises from a miasm, which generates in the human body *an eruptive fever distinct from all others, as other exanthemata are distinct*."<sup>3</sup>

The first point of resemblance, and one much insisted on by the older writers, is the *primary* nature of the eruption. In this particular it differs from the petechiæ which occur in the advanced stage of many fevers, and cannot be considered essential to them. "The petechiæ," says Bruserius,<sup>4</sup> "besides that they break out in all patients, or at any rate in by far the greatest number, as I have already said, likewise appear sooner in particular instances, generally about the fourth day, sometimes even earlier; but very seldom if ever at all delay breaking out beyond the seventh day, unless they be very anomalous, while the secondary and symptomatic ones appear much seldomer, and in fewer patients, nay, very late," &c.

Hoffman<sup>5</sup> also describes them as appearing "in nonnullis *quarto vel circa septimum diem* in dorso potissimum pectore et brachiis-cum vel sine levamine maculæ in aliis copiosiores in aliis pauciores coloris varii," &c. Modern observations are consistent with these. Thus Dr. Barker, after taking much pains to prove by a reference to older authors, that this eruption was not peculiar to the Irish epidemic of 1817-18, says, "From a comparison of many cases, I would infer that it generally makes its appearance between the fifth and seventh days inclusive of the fever," &c.<sup>6</sup>

<sup>1</sup> Ratio Medendi, vol. ii. chap. 1.

<sup>2</sup> Edinburgh Medical and Surgical Journal, vol. xxviii.

<sup>3</sup> Idem. vol. xlvii.

<sup>4</sup> Institutes, vol. iii.

<sup>5</sup> Medicinæ Rationalis. Tom. iv. p. 120.

<sup>6</sup> Dublin Medical Transactions, vol. ii.

If we refer to descriptions of the jail or hospital fever, we find Monro enumerating the fourth, fifth, sixth, and seventh as the most frequent days of the measly eruption; and Sir J. Pringle states that he frequently saw them as early as the fourth or fifth day.<sup>1</sup>

Another resemblance is presented in the phenomena attending the progress of the disease; more especially the attenuation which may be observed between the eruption and the affections of mucous membranes. In exanthematous typhus, the same dry harassing cough is observed previous to the appearance of the eruption, as in measles. On the coming out of the eruption this subsides, unless a catarrhal complication exists. Again, if the mucous membrane of the bowels be the seat of irritation, and a diarrhœa, (whether the effect of the disease or of medicine exist,) the eruption will fade. This is analogous to what has been observed in scarlatina,<sup>2</sup> and it has been urged as an argument for the free use of purgatives in typhus, that they clear the skin from spots. In the definite nature of its progress, and its disposition to terminate critically and at once, typhus resembles the exanthemata as much as it differs from the intermittent and remittent fevers with which it has been confused and compared. Neither does it appear that when once the febrile movement has commenced it can be arrested any more than the action of other morbid poisons. Most of the cases in which this is supposed to have been done, have been merely cases of strong nervous shock from exposure to infection, without evidence of the infection having been imbibed into the system.

The last resemblance upon which it is necessary to dwell, is the mode of communication.

The fact of typhus being communicated from one person to another, is a powerful argument for classing it among the *special contagions*. An examination (hereafter) of the circumstances which favour infection, will show them to be the same in both, and the time at which they become infectious seems to be the same in both, viz. at and after the period of maturation or crisis. The argument adduced by Dr. Ferriar against the humoral theory, "that neither would a patient, after recovering from a nervous fever, cease to infect others till the whole mass of his fluids were changed," is thus deprived of its weight.

The histories of patients admitted into our fever hospital, afford frequent illustrations of this fact, as they constantly attribute their infection to some neighbour, or member of their family, who has returned home cured from hospital; and there is at present in the hospital a man who has suffered severely from this cause, having lately lost his wife by a typhous fever which commenced on one of his children, who was hugged and kissed by a man upon his discharge from the hospital, after passing through a most severe typhus.

<sup>1</sup> Monro on Hospitals, p. 10; and Sir J. Pringle on Diseases of the Army, p. 299.

<sup>2</sup> By Fothergill and others.



But as evidence on a large scale is to be preferred to individual instances, let us take Dr. Perry's very strong and satisfactory testimony to the fact with reference to *both* diseases.<sup>1</sup>

Into the fever house in Glasgow are admitted cases of measles, scarlatina, and small-pox, and patients are very frequently sent in labouring under bronchitis, &c. &c. I found by experience, that when the latter class of patients were sent into the convalescent ward, where they necessarily mixed with the others, almost all who had not previously had typhus fever, were either seized with it before leaving the house, or returned soon after labouring under it. The period intervening between the time of their being sent to the convalescent ward and the attack being never less than eight days. Although means were taken to keep those recovering from small-pox, scarlatina, &c., in a separate room from those convalescing from fever, the rooms being adjoining, the non-intercourse was incomplete, and the result was, that these diseases occasionally spread among the typhous convalescents, and the convalescents from small-pox and scarlatina caught typhus." He states that "the result of a trial of the plan of keeping non-febrile cases in the acute wards till able to go to their homes was, that *not one* so detained ever caught fever in the wards, or returned with it afterwards." Dr. Perry's statement is confirmed by Dr. Stewart, who says, "In fact, scarcely one of the hundreds dismissed from the *acute* wards ever returned labouring under typhus, though they had remained for a week or ten days in wards sometimes crowded to excess, while of the few who by mistake went into the *convalescent* wards, scarcely one escaped the disease, and several died."<sup>2</sup>

Such are some of the most striking analogies between typhus and the class exanthemata; others not less important arise from a consideration of the supposed discrepancies which exist between the laws and phenomena of the two diseases.

Each writer who has opposed this classification of fever, has urged some objection or other which he considered fatal to it. We shall examine them in detail, and endeavour to show that they belong to two classes. 1, Those which apply to the exanthemata as well as to typhus; and 2, Those which *do not* apply to typhus, but to other fevers.

In both cases the argument from discrepancy must be ill-founded, as in the first the differences become analogies, and in the second, typhus, by being separated from other fevers, becomes more completely identified with the "specific contagions."

To commence with the latent period of typhus. Its variable length has been urged against the classification. That of the exanthemata appears to be equally so. In scarlatina it may extend from a few hours to twenty-one days, according to Dr. Williams and Dr. Maton. In measles, from a week to a fortnight; and in small-pox, from five to twenty-three days.

<sup>1</sup> Dublin Medical Journal, vol. x.

<sup>2</sup> Edinburgh Med. and Surg. Journal, No. cxlv.

II. The eruption, it is said, is not invariably present. This objection is not as strong as it appears, and since it is admitted that the eruption of typhus has only very lately been attentively examined as a diagnostic character of the disease, we cannot think the question likely to be illustrated by the kind of testimony which some opponents bring to bear upon it.<sup>1</sup>

The answers to this objection are, 1, It is often present, though so indistinct as to escape a superficial examination. "On such occasions," says Dr. Barker,<sup>2</sup> "*the suffusion of the eyes* is a pretty certain indication of its presence." "They sometimes," says Bruserius,<sup>3</sup> "lurk under the epidemics, scarcely perceptible, and are only seen through it on attentive examination; nay, they sometimes do not appear unless cupping glasses be applied, by which they are called out."

Similar is the observation made by Sir J. Pringle,<sup>4</sup> and repeated by Dr. Roupell, upon the arm on which a ligature had been applied for bleeding.

2. In the returns from which the comparative frequency of appearance of the eruption is deduced, there are two sources of error which have been well exposed by Dr. Davidson. The first is, that they contain a large proportion of cases *not* typhus; the other, that many of them entered hospital at an advanced stage of the disease, after the retrocession of the eruption.

Dr. Davidson observes that one fact powerfully supports the opinion that contagious typhus, in the great majority of cases, particularly in adults, is attended with the eruption, viz. that almost all the instances of fever which have occurred during the last six or seven years among the physicians, clerks, nurses, &c., of the Glasgow Fever Hospital, have been accompanied with this exanthema.<sup>5</sup>

The following remarks of Dr. Stewart on this subject deserve consideration.

"Nor can I consent without reserve to conclusions drawn from the alleged absence of eruption; for the fact I have already referred to (viz. that the eruption in typhus in Edinburgh was unheeded before 1832) shows how appearances may escape the eye of the most distinguished and practised physicians, when their attention is not particularly drawn to them. It is also well known to many, that previous to a visit which Dr. Peebles made to the Glasgow Fever Hospital, in the spring of 1835, the exanthema of typhus, then found to be of general occurrence, had neither been looked for nor registered in that institution, and was received as a new discovery."<sup>6</sup>

<sup>1</sup> Vide Dr. West's paper.

<sup>2</sup> Dublin Medical Transactions, vol. ii. Monro also remarks, that though many had no petechiæ, in all who were very bad, the countenance looked bloated, and the eyes reddish and somewhat inflamed, page 12.

<sup>3</sup> Institutes.

<sup>4</sup> Page 300.

<sup>5</sup> Essay.

<sup>6</sup> Edinburgh Med. and Surg. Journal, vol. liv.

3. We reply that the occasional absence of the eruption is in truth an *analogy*. "For," says Burserius, "as the variolous fever, or the variolous disease unaccompanied with small-pox, sometimes occurs, I should not consider it at all absurd to suppose that the petechial fever may in like manner take place without petechiæ."

In another place this author remarks: "This is generally observed to happen when they prevail epidemically. But it does not occur so frequently and decidedly to the observation of any one as that of the inoculators. For not unfrequently at the usual time after the inoculation, a fever comes on which continues several days, and then goes off without being followed by an eruption of pustules. Who would not call it a variolous fever?"<sup>1</sup>

I am acquainted with a family in which small-pox made its appearance, affecting different individuals in the following modes. One with confluent eruption, another with scanty, two with variolous fever without eruption, and another with intense vomiting and delirium, but no subsequent fever or eruption.

The same occurrence of a peculiar fever without eruption, has been remarked in epidemics of measles, by Sydenham and others. Rayer states that Guersent has observed individuals in families where measles prevailed, exhibiting all the other symptoms of the disease except the eruption, and that he has himself several times seen cases in which the eruption was incomplete, and which might have been referred to the morbillary fever of Sydenham.<sup>2</sup>

Every one who has had any experience of epidemics of scarlatina, must have observed fever and sore throats of the same character as that of scarlatina, but without eruption, occurring in families in which this disease prevailed. Rayer quotes the testimony of a number of authors upon the subject, and Dr. Tweedie introduces it as a variety of the disease into his classification. This scarlatina sine exanthemate is very frequently met with in practice.

III. A want of uniformity of the character and time of appearance of the eruption has been alleged.

"Of the varying characters of the eruption," says Dr. West,<sup>3</sup> "almost every quotation has afforded an illustration, and we have seen the date of its appearance vary from the second to the seventeenth day."

We are by no means convinced that the subject has been *illustrated* by Dr. West's quotations, which appear to be descriptive not so much of typhus as of every other variety of fever. On the other hand, testimony is not wanting of observers who have explained these apparent irregularities in the character and periods of the typhus eruption, and reconciled their apparent inconsistency with an exanthematous theory of fever.

Such we meet in the following passages from Burserius's admir-

<sup>1</sup> Institutes, vol. iii.

<sup>2</sup> On Diseases of the Skin.

<sup>3</sup> On Exanthematous Fever. Edinburgh Medical and Surgical Journal, No. cxliii.



able chapter on the petechial fever. "Le Roy also observes that there is some distinction between the primary and secondary petechiæ, which consists in the difference of their colour, namely, that the former are of a palish red and rosy colour, and in general break out in great numbers, principally on the loins and legs; that the latter, on the contrary, are generally of a purple colour, like deep red wine, and are sometimes also brown or black, and fewer in number."

But we must also remember that the primary ones break out soon, and when they are epidemic, appear not only in all affected with the same disease, but are likewise very frequently combined with other diseases called intercurrent ones—(for these last are not always wanting, as some contend)—while on the other hand, the secondary ones break out later, and generally about the height, or towards the end of the disease, and not in all patients, but only in those whose blood is so vitiated as to become almost putrid, and occasion gangrenes here and there on the skin, or being thrown into violent commotion by a heating regimen and medicines, is effused into the spaces of the skin, *but not by the wisdom of nature endeavouring to free herself from the noxious miasma*. Hence I would say that the primary differ from the secondary petechiæ, because the former arise from a peculiar and poisonous miasma, and the secondary from the crisis of the blood being deranged by the violence of the disease, or from its increased motion, or lastly, from a heating regimen having been employed." Such also we meet in Dr. Staberoh's paper on the eruption attending epidemic fever. In which he shows that not only do petechiæ of the ecchymotic or secondary kind occur after and *apart* from the exanthema, but that spots of these are capable of being converted into ecchymotic spots.<sup>1</sup> Attentive observation has convinced me that not only are the above statements correct, but that we may add that a third variety of late petechiæ occur in cases in which, from diarrhœa or hypercatharsis, in the beginning of fever the exanthema lurked under the epidemics. The conversion of this *indistinct eruption* into ecchymosis taking place, or the latter being superadded in the course of the disease, *and appearing to be primary*. A fourth variety is thus alluded to by Dr. Peebles: "Petechiæ may be mixed with the exanthema, and in some epidemics the exanthema has been prevented from showing itself by the disease passing so rapidly from the sthenic state to the putrid, that it has not had time to make its appearance."

Of course under any of the foregoing circumstances the late appearance of a petechial eruption is no argument, for a want of uniformity in that of the exanthema. The frequency of occurrence of these secondary petechiæ is only an additional reason for believing that the two forms have been by many writers confounded together.

<sup>1</sup> London Medical Gazette, vol. i. N. S. p. 973.

IV. It is objected "That the disease often occurred more than once during the lifetime of an individual."

The objection assumes that typhus confers no immunity from subsequent attacks, and that the exanthemata do confer an immunity. The answer is, that experience warrants our belief in a considerable power of destroying the liability to subsequent attacks in typhus, and that, though there can be no doubt of the exanthemata possessing this power, exceptions to it are frequent in all of them.

It must be admitted that in this country there is a general belief in the protecting power of a seasoning or initiatory fever, and though we rarely meet with a medical man who has not had typhus, we certainly meet with few indeed who have had it more than once. The nature of the subject does not admit of very precise proof. We can only obtain the *general results* of experience. Dr. Barker<sup>1</sup> states as the results of his, "that he has for some time entertained the opinion that sufferers from fever attended with this eruption, if they are not altogether secured by it from a second attack, are not at least so liable to it as those who have had a fever of the ordinary kind; and, though he frequently made the inquiry, he never found a patient in whom this symptom was distinct who had suffered from the same fever on any former occasion." Dr. Perry<sup>2</sup> states, as the result of an extensive series of observations, his opinion, "that typhus generally is taken but once in a lifetime, and that a second attack does not occur more frequently than of small-pox, and less frequently than of measles or scarlatina." Dr. Davidson states that of 609 patients in the Glasgow Fever Hospital only seventy-four stated that they had ever had fever previously. He justly observes that when we take into account the various diseases which are confounded with typhus, this small number can be easily accounted for.

But the protective power of the exanthemata has been much overrated. Three instances of second attacks of small-pox came to my knowledge in this county very recently. In two of them the patients had suffered the disease from inoculation very few years before. In one in which the inoculated disease was severe, a most confluent eruption accompanied the second attack seven years after. Another instance has been related to me of a lady living in this country who has had the disease three times. Dr. Roupell refers to the case of one who had it seven times.

Instances of second attacks of measles are given by Dr. Bailie, who attended five children in May, and again in the following November; by Dr. Webster,<sup>3</sup> and by Rayer, who states that he met with three instances of second attacks of measles in the interval between the publication of the first and second editions of his work. The remarkable case of a second attack by Dr. Graves<sup>4</sup> should,

<sup>1</sup> Dublin Medical Transactions, vol. ii.

<sup>2</sup> Dublin Med. Journ. vol. x. and Edin. Med. and Surg. Journ., Jan. 1836.

<sup>3</sup> Medico-Chirurgical Transactions. Second Series. vol. iv.

<sup>4</sup> Dublin Medical Journal, Nov. 1840.

perhaps, be termed relapse into measles. The second eruption appeared twenty-one days after the commencement of the first illness, in which the eruption had been copious and severe.

Cases of second attack of scarlatina are stated by Roupell to be not at all uncommon. Several have fallen under my own observation.

V. The liability to relapse in cases of typhus has been urged as an objection to the classification by Harty and others. It might be replied that cases of measles, such as that of Dr. Graves just referred to, and cases of *reversio*, as it is termed by Rayer, after scarlatina, would tend to show that the exanthemata are not exempt from relapse. But the true answer is that typhus is *peculiarly exempt* from relapse. Two kinds of cases are erroneously considered such: 1st. Relapses from typhus into fever symptomatic of a visceral irritation—generally gastro enterite.—“I am persuaded, says Cheyne,<sup>1</sup> “that obstinate and fatal relapses after typhoid fevers are often attributable to inflammation and, perhaps, ulceration of the villous coat of the intestines.” And Broussais asserts that “when the frequency of pulse in ‘convalescence’ does not diminish, and the strength does not increase, it may be suspected that a form of latent inflammation exists. It may be discovered by permitting an excess which generally changes this frequency into a *real fever*, and develops the pain of the irritated part;”<sup>2</sup> but, 2dly, cases of fevers not typhoid will under exposure to infection relapse into typhus. Dr. Davidson gives a tabular view of the relapses in the Glasgow Fever Hospital among 686 cases, in which no case of relapse from typhus into typhus occurred, but two of febricula and one of intestinal fever into typhus.

In 500 cases of fever admitted into the Navan Hospital in 1840, two cases only of relapse into typhus occurred, both were cases of febricula, which after a few days were sent to the convalescent ward, where they relapsed into maculated typhus, one in four days and the other in fourteen days after their removal thither.

VI. Lastly, the following extraordinary objection is put forward by Dr. West:—<sup>3</sup>

“The type of the fever itself varies, being sometimes intermittent, sometimes continued, changing from the one to the other form, and being occasionally converted into other diseases.”

In other words there is no such disease as typhus!

To this the supporter of the speciality of typhus replies, that the disease is here, and in numerous quotations throughout the paper, confounded with other fevers; typhoid it may be in their nature, or becoming so in their progress, not arising from an animal infectious poison, but from a variety of sources, which contain a variety of poisons, the identity of *any one of which* with the *typhus*

<sup>1</sup> Dublin Hospital Reports, vol. i.

<sup>2</sup> Chronic Phlegmasia, vol. ii. p. 53.

<sup>3</sup> On Exanthematous Typhus.



poison is a matter in dispute, and to be argued upon the conclusion of our investigation into these sources in the following chapter.

Meantime the pertinent remarks of Dr. Copland, upon this subject, are not unworthy the notice of those who rely for the means of drawing accurate distinctions upon such sources as Dr. West has explored :—" *True or contagious typhus* has been confounded with synchoid and nervous fevers on the one hand, and with putrid or malignant fevers on the other. It has been already stated that putridity or malignancy, not only may characterise a particular form of fever or certain epidemics even at an early period of their course, but also, owing to various contingencies, may take place in advanced stages of any other fever. As the circumstances favouring the generation and spread of typhus are often such as also tend to develop those changes which have been usually named putrid or malignant, and as these changes are frequently observed in the latter stages of typhus—the symptoms distinguishing this fever becoming associated with, or followed by, those indicating the putro-adyynamic state—so has it been often confounded with other fevers in which this state has predominated more or less. If we refer to the numerous histories of epidemic typhus, recorded by writers from the close of the fifteenth century up to the present time, we shall find that although many of these, owing to the concurrence of circumstances developing a putrid or malignant disease, were instances of fever either identical with or very closely resembling that which I have described as such in the preceding section, yet many others, or even the majority, were true typhus, in which the putro-adyynamic state was either early or predominantly developed. The exanthematous eruption, characteristic of typhus, being succeeded or accompanied by the petechiæ, indicating the approach of the septic condition, and being either mistaken for them or for an eruption of miliaria. Owing to this circumstance, especially typhus, low, nervous, and putrid fevers, have been very generally confounded together."

The reader of the foregoing, and many other passages in Dr. Copland's admirable article on typhus, must be startled with the following passage in Dr. Roupell's recent work on the disease, when he finds that Dr. Copland is not like Peebles and others, who have described exanthematous typhus, passed over in silence, but is actually mentioned by name as belonging to the authors referred to.

"In the above description *typhus* is considered to belong to the continued fevers. It is looked upon by the most recent authors, in this and other countries, not as an individual disorder, but as one into which others may be and frequently are converted?" (page 5.)

Here for the present we leave the subject, since that portion of the argument for the classification of typhus with the exanthemata, which is derived from the differences between it and other continued fevers, will properly come under consideration when discussing the identity of typhus and typhoid fever.

## CHAPTER II.

Of a Fever Poison, generated during the Decomposition of Dead Organic Matters.

The difficulties attending an examination into this part of the inquiry into the sources of fever are very great, and are confessed by all who are familiar with the conflicting statements advanced on the subject. Our difficulties are increased by the indeterminate character of much of the evidence offered in proof of the paludal origin of fever, some of which not only claims to prove the power of such sources to cause continued, but *infectious* fever—by the fact that much of this evidence is moreover inadequate, as it proves only the occurrence of fever in situations and among persons which might be considered equally obnoxious to contagion as to miasm, and by the silence, or mysterious, or contradictory language of those to whom we might look for assistance and direction in a scrutiny of the mass of conflicting testimony, from which our conclusions are to be drawn.

Thus, Dr. Christison says, "the great questions involved in the investigations into the causes of continued fever are three in number:—Does the disease originate in infection? Does it originate in other causes? Granting that it does originate in other causes, may such fevers propagate themselves by infection? It will be seen that they cannot be all answered by any means with equal confidence;" and, accordingly, while he is full and illustrative on the subject of contagion, he treats of other causes in a most cursory and unsatisfactory manner, and while he admits that "the general conclusion from the whole facts seems to be that a disease, undistinguishable from true infectious fever, may sometimes arise without infection," adds, "that on descending from the general question to the more special one—what the other cause or causes of fever may be?—the difficulties are greatly increased, indeed they become insurmountable, without such limited and vague facts as are at present possessed on the subject," and "it appears a needless waste of time and labour to attempt any thing further on this head."

Nor are we more enlightened by Dr. Davidson, who, while he states that he is not prepared to assert that febrile affections may not, under peculiar circumstances (what he does not inform us,) arise from paludal sources, effectually excludes them from consideration by putting forward the following conclusions under the head of "Alleged sources of continued fevers, *not* typhoid."

"From a consideration of the whole evidence that might be adduced respecting this point, it may be drawn as a conclusion, that although putrid matters when injected into the veins of animals cause death under symptoms similar to those of typhus fever, yet that the effluvia arising from similar matters do not, under ordinary circumstances, produce any deleterious effects on man." Again—

"Before concluding this part of the essay we shall notice an hypothesis, which has lately been somewhat confidently brought forward to account for the prevalence of typhus in some large cities, viz. : that a peculiar malaria is generated by the animal and vegetable filth, which accumulates along the sides of rivers running through large towns, and that the inhabitants who live in their immediate vicinity become thereby subject to fever. We are quite aware that very disagreeable and sometimes fetid effluvia occasionally arise from such situations, particularly during hot weather, but that it is capable of causing continued fever has not even been rendered probable by any satisfactory evidence."

We have it stated upon high authority that gaseous contagions contain organic matter in a state of decomposition or progressive change. We have it also announced that from certain decomposing animal and vegetable substances, organic matter in a state of "progress to decay" is evolved, which, when collected and retained in a manner similar to the former, completes the stage of decomposition, or, in other words, "putrefies."

By evidence of the most unexceptionable kind the former of these is proved to be capable of communicating the *state of change*, of which it is the subject, to the healthy human organism.

We have to inquire whether the analogy of *action* of these bodies is as perfect as the analogy of *condition* appears to be, and, whether, "when the process of respiration is modified by contact with a matter in the progress to decay, when this matter communicates the decomposition, of which it is the subject, to the blood—disease is produced."

We shall first state the analogy of condition of the *tangible poison*, evolved from decomposing organic substances, in the words of Dr. Southwood Smith—not only because it is clearly stated by him (so far as relates to its tangible existence,) but, also, because this passage has furnished the text for some of the objections which we shall have to consider<sup>1</sup>:—"It is known to every one that the putrefaction of vegetable and animal matter produces a poison, which is capable of exerting an injurious action on the human body. But the extent to which this poison is generated, the conditions favourable to its production, and the range of its noxious agency, are not sufficiently understood and appreciated. It is a matter of experience, that during the decomposition of dead organic substances—whether vegetable or animal—aided by heat and moisture, and other peculiarities of climate, a poison is generated, which, when in a state of high concentration, is capable of producing instantaneous death by a single inspiration of air in which it is diffused. Experience also shows that this poison even when it is largely diluted by an admixture with atmospheric air, and when, consequently, it is unable to prove thus suddenly fatal, is still the fruitful source of sickness and mortality—partly in proportion to

<sup>1</sup> Poor-law Commissioners' Fourth Report, page 130.



its intensity, and partly in proportion to the length of time, and the constancy with which the body remains exposed to it, &c.

“But this poison was too subtle to be reduced to a tangible form. Even its existence was ascertainable only by its mortal influence on the human body; and although the induction commonly made as to its origin, namely, that it is the product of putrefying vegetable and animal matter, appeared inevitable, seeing that its virulence is always in proportion to the quantity of vegetable and animal matters present, and to the perfect combination of the circumstances favourable to their decomposition, still the opinion could only be regarded as an inference. But modern science has recently succeeded in making a most important step in the elucidation of this subject. It has now been demonstrated by direct experiment, that in certain situations, in which the air is loaded with poisonous exhalations, the poisonous matter consists of vegetable and animal substances in a high state of putrescency. If a quantity of air in which such exhalations are present be collected, the vapour may be condensed by cold and other agents, a residuum is obtained, which, on examination, is found to be composed of vegetable or animal matter in a high state of putrefaction. This matter constitutes a deadly poison. A minute quantity of this poison applied to an animal, previously in sound health, destroys life with the most intense symptoms of malignant fever. If, for example, ten or twelve drops of a fluid, containing this highly putrid matter, be injected into the jugular vein of a dog, the animal is seized with acute fever, the action of the heart is inordinately excited, the respiration becomes accelerated, the heat increased, the prostration of strength extreme, the muscular power so exhausted that the animal lies on the ground wholly unable to stir or to make the slightest effort, and after a short time it is actually seized with the black vomit, identical in the nature of the matter evacuated with that which is thrown up by a person labouring under yellow fever. By varying the intensity and the dose of the poison thus obtained it is possible to produce fever of almost any type, endowed with almost any degree of mortal power.”

In this last sentence we recognise the echo of Magendie's questionable assertions; the preceding statements are confirmed by the account of experiments upon “*le mauvais air*,” given by Devergie:<sup>1</sup> “The gas, which is disengaged from putrefying animal matters, extracts with it a particular odour, infectious ‘*infecte*,’ characterised by the general term putrid odour. We attribute this odour to miasma, that is to say to a cause void of meaning, because we are ignorant of the nature of the object which it represents.

“Guntz has endeavoured to enlighten the phenomenon by the following experiment: he placed a bell glass over a portion of a putrefying dead body, in such a manner as to permit the air to

<sup>1</sup> *Medicine Legale*. Tom. i. p. 100. I am indebted to my friend Dr. Aldridge for referring me to this account of experiments on the subject.

penetrate, he submitted the apparatus to a temperature of 26° Cent. (equivalent to about 78° Far.) and, after a period sufficiently prolonged, he suddenly cooled the bell glass; immediately the product of the vapour assembled itself into drops, which evolved a strong odour of miasma, he treated these drops with chlorine, when the odour disappeared. He was thus led to suppose that the gas in escaping from the putrefying animal matter carried with it the vapour of water combined with a certain quantity of animal matter, very minutely divided, and this constitutes what has been named miasma.

"This is not the only experiment calculated to lead to this opinion—others have been made with respect to vegetable matters. Moscati entertained the first idea of condensing the water dissolved in the atmosphere, for the purpose of detecting the principle which occasioned 'le mauvais air.' He suspended at some distance from the soil mattresses full of ice; the water which became deposited upon their surfaces condensed itself readily, when limpid it presented many small flakes which possessed all the essential properties of *animalised matter*. After a few days they putrefied completely. In the course of the year 1812, M. Rigaud undertook, in the marshes of Languedoc, a series of essays directed to the same end. He condensed dew on glasses, and the water which he obtained by this means presented all the phenomena obtained by Moscati.

"In 1819 M. Boussingault observed that *sulphuric acid* placed in the proximity of a well, in which he had caused animal matter to putrefy, *blackened* very rapidly. He repeated this experiment in many infectious places, and found constantly that the coloration of the acid was more prompt according as the air was more infectious," &c.

The inference naturally deduced from such experiments as the foregoing, taken in conjunction with the fact of the occurrence of fever in situations where these putrid exhalations have been found to exist—namely, that they contain a *fever poison*—has been met by numerous objections. The principal seem to be the following :

1. That a mephitic poison is confounded with the fever poison.
2. It is denied that these sources ever generate fever, because the number of cases does not bear a sufficient proportion to the number of instances of exposure, and because they generate several diseases differing in their nature from fever, and it would amount to a confounding of fever with these,<sup>1</sup> if we attributed its origin to the same poison.
3. It has been objected to the evidence of the frequent occurrence of fever from this source—that it is furnished from the experience of persons who deny the infectiousness of fever, and is,

<sup>1</sup> "Dr. Smith illustrates and supports his doctrine of the malarial origin of fever by referring to facts which relate merely to periodic fevers; and he maintains the identity of the 'fever poison' of this country with the poison of plague; wherefore, on the principle, that things that are equal to the same thing are equal to one another—plague and ague are generated by the same poison!"—*Forbes's Review*, No. 21, p. 13.

therefore, suspicious;<sup>1</sup> that in the recorded cases malaria and contagion have been confounded; that it amounts only to a proof of the frequent coincidence of fever and the effluvia from filth, and does not prove that the former stands to the latter in the relation of an effect to a cause; that, granting continued fever is ever thus produced, it is not contagious or typhus fever, &c.

I. It has been said<sup>2</sup>—"If the statements of Dr. Smith were put into the simple form of the only proposition which they really contain, they would amount merely to this—that exhalations from certain putrescent matters have the power of producing both asphyxia and continued or typhus fever; the former of which is a result familiar to all, and the latter, a mere assertion, deriving a little hue of probability from its juxtaposition to a known truth. There is a wide difference between the asphyxia, which is caused by mephitic gases, and typhus fever,—a difference which can never be explained, as Dr. Smith attempts to do, by a reference to the diversity in the doses of the poison. We presume that if a few doses of the poison, in its less potent shape, were sufficient to create typhus fever, *a fortiori*, such a quantity of it in a more concentrated form, as would be capable of producing a state of asphyxia not ultimately fatal, would commonly at least leave the sufferer for days or weeks in the toils of a highly dangerous fever, yet the reverse is the case, as the histories of mephitism amply demonstrate."

So they doubtless do, and they, moreover, show that in some cases of recovery from mephitism, a disease, apparently the effect of a morbid poison, followed, though *not* fever.

But this writer has, like Dr. Smith, confounded the action of two poisons of different kinds—an inorganic poison, sulphuretted hydrogen, and a morbid poison, whose action depends not upon its chemical qualities, but upon the existing condition of its particles, they being at the time of their evolution in a state of decomposition or transposition.

The advocate for the malarial origin of fever does not regard the fever poison as the product of *extreme putrefaction*, capable of causing mephitism or fever, according to the dose in which it is applied. But he holds that during the *progress* to decay of organic substances, matter in a state of decomposition is evolved which is capable of communicating its state to the organism with which it may be brought into contact, while, on the completion of the process of decomposition, the morbid poison ceases to be evolved and the mephitic poison is generated.

A perfect analogy to this is found in the effects of decayed sausages, which, according to Christison, "are poisonous only at a particular stage of decay, and cease to be so when putrefaction has advanced so far that sulphuretted hydrogen (the mephitic gas) is

<sup>1</sup> Vide Dr. Christison's article—Fever. Library of Medicine.

<sup>2</sup> Forbes's Review, *ut supra*.



evolved." True, in mixed sources, and those which are receiving daily additions of new matters, the morbid poison may co-exist with the mephitic poison, and the latter may occasionally, by its sedative effects upon the nervous system, assist the operation of the former; but they are essentially different in their nature and action.

The second objection—"an alleged want of proof that the fever poison is ever generated in such sources"—rests, 1st, upon the relatively small number of cases of fever so produced, compared with the activity of contagion; and, 2dly, upon the fact that several diseases of different kinds, from *tic-doloureux* up to plague, are attributed to miasmatic effluvia. Can we (it is asked) believe that they are all owing to the same poison?

The first of these grounds is urged against decomposing animal matters; the second chiefly against mixed sources, as sewers, banks of rivers, &c.

It is true that very few observations exist which can be said to prove the occurrence of fever from exposure to animal putrefaction—still, some such cases have occurred. The following is referred to by Dr. Christison as an unexceptionable one:—<sup>1</sup>

"An American merchant-ship was lying at anchor in Whampoa road, sixteen miles from Canton. One of her crew died of dysentery. He was taken on shore to be buried; no disease of any kind had occurred in the ship from her departure from America till her arrival in the river Tigris. Four men accompanied the corpse and two of them began to dig a grave. Unfortunately they lit upon a spot where a human body had been buried about two or three months previously, as was afterwards ascertained,—the instant the spade went through the lid of the coffin a most dreadful effluvia issued forth and the two men fell down nearly lifeless: it was with the greatest difficulty their companions could approach near enough to drag them from the spot and fill up the place with earth. The two men now recovered a little, and, with assistance, reached the boat and returned on board. . . . One of these men died on the evening of the fourth day, and the other on the morning of the fifth, after symptoms of malignant petechial fever (the petechiæ occurring on the fourth day.)

"In eight days after the opening of the grave one of those who were not engaged in the work was attacked with the same symptoms as his companions, and the fourth had a slight indisposition of no very decided character."

It is to be remarked, that in the above case two circumstances were present which we shall see have not always existed in the negative instances, brought forward to prove that this is not a source of fever; namely, "*confinement* of the effluvia," and a *not very advanced* stage of putrefaction. Ferriar, who did not consider the exhalations from putrid animal matters a source of fever,

<sup>1</sup> Medico-Chirurgical Review, for Jan. 1825, p. 203. Dr. Christison also refers to the Mem. de la Soc. Royale de Med. 1. 97.

says—<sup>1</sup>“It appears from some late observations made on altering the vaults of a church in France, that the *confined effluvia* of putrid bodies produce fever when brought into action. Perhaps this is the solution of the question.” <sup>2</sup>“Fourcroy states, that the grave-diggers informed him that the putrid process disengages elastic fluid, which inflates the abdomen and at last bursts it; that this event instantly causes vertigo, faintness and nausea in such persons as, unfortunately, are within a certain distance of the spot where it happens, &c.” In the exhumations, conducted on such a large scale at the cemetery of the Innocents, and quoted by Bancroft and others in proof of his position, neither of these conditions could have existed, since no interments had been allowed for six years previously.

In many of the cases also related by Mr. Walker it is mentioned that the bodies had been buried for years, or were in an advanced stage of putrefaction; under these circumstances mephitism was produced—but not fever.

The other part of this objection, namely that so many different diseases are ascribed to this source—can they all be the effect of the same poison? can only be answered by supposing a variety of morbid poisons to be formed together or consecutively in the same source. Several considerations render it probable that this is the case in some malarial sources.

1. The progressive nature of the changes which the decomposing body undergoes, and the different circumstances under which the same organic matters (undergoing decomposition) may be placed in different places, or at different times. There is nothing improbable in the supposition, that the same source may at one time give origin to the poison of ague, and at another to the poison of fever.

2. The fact that an individual exposed to these sources will frequently become affected with two diseases. These will usually follow one another at a short interval. “In the vast horde of cases,” says Dr. Addison,<sup>3</sup> “which the river side is continually sending forth, synochus and typhus are of frequent occurrence, and these are frequently followed, when the patient is convalescent, by well-defined agues.” Sir H. Marsh has noticed the same occurrence in the epidemic of 1826, in Dublin, and it is well known that after this epidemic the hospitals of that city were filled with cases of ague. This is perfectly analogous to what happens from exposure to two morbid poisons, the one which has the shortest latent period takes precedence and is followed by the other, as in the case related by Dr. Williams of a boy who was inoculated at the same time with the virus of measles and cow-pock. The cow-pock, first ran its course, and was then followed by measles. In the same way it is very possible that the poison of ague, imbibed at an early part of

<sup>1</sup> Medical Histories, vol. i. On New Contagions.

<sup>2</sup> Walker's gatherings from Grave-yards, p. 124.

<sup>3</sup> On Malaria. London Medical Gazette,—vol. iii. new series.

the year, may lie latent until the conclusion of a continued fever received many weeks later.

3. This power of generating different diseases, is alleged of those sources in particular which contain a *variety* of organic matters, and which are in a state of constant change from the superaddition of new materials or from atmospheric changes—such are sewers, the banks of rivers, &c.—and it is to these that the great body of evidence, as to the frequent production of fever, applies, and not to the regular, uniform, and spontaneous decomposition of any single portion of animal matter, however great its bulk.<sup>1</sup>

SECT. II.—In order to obviate the objections urged against the evidence of the frequent occurrence of fever from malarial sources,—namely, that it has been confounded with contagion, and that, at all events, the evidence proves no more than the frequent co-existence of filth, fever, and poverty.

We shall select only a few cases which have occurred under circumstances unfavourable to the supposition of such a cause as contagion, and the histories of which present contrasts to that of contagious fever in some of the following particulars.

I. The class of persons affected, not those usually obnoxious to contagious fever unless under circumstances of prolonged exposure.

II. Occurring without the presence of any of the aids to contagion and at an opposite season of the year.

III. In localities in which contagious fever does not prevail.

IV. Spreading in spite of the preventive measures, which are found to check the diffusion of contagious fever.

“In great towns,” says Christison, “cases are met with during the intervals between epidemics, and in a station of life where epidemic fever in epidemic seasons of the worst kind is seldom witnessed. A fever of this description, tedious in its course, characterised by much nervous and muscular depression, without any particular local disturbance, and, especially, without the marked

<sup>1</sup> If the subject admitted of an explanation purely hypothetical we might draw an analogy, not destitute of plausibility, between the action of human contagion and putrefaction in this particular; and we might suppose, that as in fever generated spontaneously in the human body, there does not seem to exist any power of communication by infection, so in dead animal matter the product of any single mass of spontaneous putrefaction is not a fever poison, but that this is generated by the exposure of *fresh* dead organic matter to the contagion of the former. For this hypothesis to be consistent with the facts, the following should be the various consequences of exposure to putrefactive decomposition:—

1. From exposure to a single mass of animal matter undergoing spontaneous putrefaction—no fever.

2. From exposure to the emanations from substances added to the former disease, varying in intensity in proportion as circumstances were more or less favourable to rapid communication of the “contagion of decay” from decomposing to recent organic matter.

3. From successive exposure of a number of individuals to successive additions of organic matter, (under above circumstances,) a number of cases of disease.



disorder of the functions of the brain which distinguishes most cases of epidemic typhus and synochus, was so prevalent among the better ranks in certain streets of Edinburgh some years ago, at a time when fever was not prevalent among the working classes, that a general impression arose among professional people of the existence of some unusual local miasma. A great variety of parallel facts might be referred to—all leading to the general conclusion, that a disease if not identical with, at all events closely resembling, synochus and typhus as described above, may arise without the possibility of tracing it to communication with the sick. A statement of this kind acquires great weight in the instance of such a visitation of disease as that just alluded to, which prevailed among people in easy circumstances in a great town."

Very similar is the testimony of Dr. Cheyne:—"For several years the fever appeared in families only in solitary instances, or if more than one were affected they were seized nearly at the same time, but it did not extend so as to lead us to think that it propagated itself. We were unable to assign the cause of the disease further than that we observed in several houses, in which our patients lay, that fetor which is discoverable when a sewer is choked, and, in some instances, upon enquiry it was found that the sewer leading from the house had been improperly constructed and neglected."

A similar instance of fever, apparently caused by defective sewerage, came under my observation recently in the house of a gentleman of fortune in this county. For a longtime an unpleasant odour had been remarked in several parts of the mansion, more especially near this gentleman's study and in the men servants' sleeping apartment. The poisonous effects of the malaria were first produced in the form of obstinate dysentery in one of the female servants. Then the owner of the mansion was attacked with what he at first supposed was mere biliary derangement, but which rapidly assumed all the characters of severe gastric fever, becoming attended towards the close with purple petechiæ and terminating fatally on the 11th day.

About the same time two men servants were seized with symptoms of fever. In one it was cut short and in the other it ran its course, ending favourably about the 11th day.

Two other persons who came to the house on business (from the neighbourhood) and who remained in it for a few hours, were seized with the same fever, which ran through its course at their own houses but without extending to other individuals.

After this lamented occurrence the cause of the effluvium was searched for and found to be a leakage of the soil pipe of one of the water-closets, which had allowed the filth to percolate through the wall and exhale into the atmosphere of the house. This exhalation was also much favoured by the warm temperature kept up in the house by heated flues.

About the months of October and November, 1839, I was repeat-

edly consulted by the inmates of a large establishment in the neighbourhood of Navan, on account of different forms of gastro-enteric affection, especially diarrhœa and dysentery. So many instances occurred at intervals, (in some cases of weeks,) and the general resemblance was so great, that I thought they must arise from some local cause, and I expressed a strong suspicion that some source of malaria existed in the house or immediate vicinity. The house itself was large, airy, and commodious, so that our inspection was directed rather to the immediate neighbourhood, and it was thought that the cause had been discovered in an old sewer which had been laid open in the course of some building operations. The closing of this was not, however, attended with the effect of stopping the endemic affection, though it gradually ceased after about a dozen people had been attacked. The following spring was remarkably dry, scarcely any rain having fallen for about six weeks; toward the close of this period an effluvium of a very disagreeable nature became perceptible in some parts of the house, and at the same time—within a day of each other—two of the inmates were attacked with exquisitely marked typhus, attended with profuse measly eruption, and in one of the patients with violent delirium. Every circumstance rendered the existence of contagion in either case highly improbable, I might almost say impossible, and on my again expressing my strong conviction that some form of malaria was the cause of the fever, I was informed of the effluvium perceived in some of the passages, and also of the fact that in the original construction of the water-closets they had been made to depend for their supply of water upon a cistern of rain water, which, of course, had been for weeks empty during the present spring and preceding autumn. These cases did not spread, and all traces of indisposition were removed by making the required alterations for ensuring a constant supply of water.

In the month of October, 1839, I attended a respectable man who resided in a large and airy mansion, as “care taker,” during the absence of the family upon the Continent. His illness had come on slowly and insidiously, but, when I saw him, had all the characters of bad remittent fever, attended with much abdominal congestion. This it was attempted to relieve by leeches, &c., but it increased and led afterwards to large evacuations of blood from the bowels. He recovered slowly and with difficulty. At the time I saw him he was lying in the basement story of the house.

A few weeks after the return home of the family, the *butler* was attacked with symptoms of gastro-enterite and slight jaundice. He recovered partially in a few days, but in the course of a week after was suddenly seized with complete loss of muscular power, paleness and coldness of the surface, sickness of stomach, &c., followed by vomiting of a dark olive fluid, and in two days by large evacuations of tarry blood from the bowels, hiccup, subsultus, &c., while the skin was covered with vibices and black petechiæ—some of them of the size of large shot. The fever which followed had no percep-

tible remissions and perfectly accorded with the descriptions of putrid malignant fever by Huxham and others. The striking resemblance of some of the symptoms of this case to that which had occurred in the same place more than a year before, led me to attribute both to a common cause and to enquire for the source of the malaria. The following were the facts ascertained:—In a room in the basement story, occupied by the last patient and in which he had latterly sometimes slept, was a sink emptying into a pipe, which communicated at the distance of about ten feet with the main sewer of the house—into which the contents of two water-closets passed. This sewer was very large at its termination and when the wind blew from that direction towards the house, there being no smell trap under the sink, the effluvium of the sewer was carried up into this room and became so insupportable that the patient used to stuff the aperture with a piece of rag when retiring to bed. Upon inquiry I was informed that the first patient had frequently before his illness remarked the same fetid effluvium. It is worthy of remark that the sewer had *not* been cleaned in the interval between the two cases.

The following case, very similar to the last in its nature and origin, I give upon the *suspicious* authority of an anticontagionist: "I attended," says Dr. Armstrong,<sup>2</sup> "a very respectable tradesman, labouring under a remarkable bad attack of typhus fever. It was such a case as would have been called plague in the time of Sydenham. He had knotted glands and carbuncles, and black petechiæ. He was one of four or five individuals who had transacted some business in a nobleman's kitchen; a filthy fluid had overflowed that kitchen; he was sickened at the time, and in common with *all* the others had an attack of typhus fever."

If we looked about for a large town less liable to contagious fever than others we might probably find it in Birmingham,—yet here endemial causes, of the kind which Dr. Davidson has pronounced inadequate to this effect, have produced fever. A good instance for illustration is found in Dr. Ward's account of an endemic fever, which prevailed in certain localities in Birmingham in the *summer* of 1837.

<sup>3</sup> "The river Rea, that separates Birmingham from its suburb Badesley and serves as a cloaca maxima to both, carries its filthy stream onward, partly to turn a mill and partly to fill a mill pond. During the drought which prevailed last year the water was very low in the main stream and mill pond, and the mills not being regularly worked became quite stagnant and offensive. The back stream also became dry and showed its mud banks, that were only occasionally wetted by a flush of the washings of the town after a shower, or by the small surplus accumulated during the cessation

<sup>1</sup> See Christison, *ut supra*.

<sup>2</sup> Lectures by Rix.

<sup>3</sup> Provincial Medical Transactions, vol. 6.



of the working of the mills. The exhalations from the half dried mud and putrid water were so disagreeable at night as to nauseate the more delicate inhabitants of the adjoining streets, and soon produce disease in the form of typhoid fever of an infectious (?) character." He goes on to state that about 50 cases—some fatal—occurred in the immediate vicinity of the stream, and "still lower down the stream, where the water was as black as ink, there were 13 pauper cases in one yard, and many others, both pauper and private, along the same line." That this fever was owing to the state of the stream is proved by the disease being confined to the locality, the small number affected in so large a population as Birmingham, the season of the year, and the exemption of this town from the causes which aid contagion—these are well summed up by Dr. Ward.

"There is a difference of nearly 200 feet in the elevation of different parts of the town. The streets and the courts or yards in which the mechanics live are wide and airy in general; fuel is cheaper than in any other large town in England; the water is excellent—and till within the last year there has been but little distress."

We have already adduced the effects of seclusion of the sick in proof of the infectiousness of typhus. In the fever arising from endemial sources this measure has no such influence. I was much struck with this fact when making some investigations as to the source of a fever, which prevailed in the summer of 1839 in a hamlet attached to a flax manufactory near this town, from which a considerable number of cases had been sent to hospital in the months of April, May, and June. The object of an examination which I made of the place was to obtain satisfactory instances of contagion, but I soon found that no such evidence was to be procured. For the intervals between the illness of different members of the families were too irregular to admit of communication from one to another. Thus, in one house the first case sickened on the 2d of April, and the second on the 5th. In another, more than three months intervened between the first and second cases. And in several families in which the first case had been early removed to hospital, the second had sickened before the patient's return. Besides, there was too much cleanliness and comfort: several of the houses had been repeatedly white-washed during the time that the fever was going through the family, and the inmates were all well off—being employed in the neighbouring factory.

Several things convinced me that this fever had a malarial origin. The hamlet was built in the form of two parallel streets, terminating in a large open space, in front of which were twelve houses looking northeast. This space had no drainage and was full of shallow pools of black putrescent water, into which the inmates daily threw cabbage leaves, &c., to rot for manure. In this country the east and northeast winds prevail for the first three months of summer—April to June—and in consequence the inhabitants of the twelve houses described were peculiarly obnoxious to the ema-

nations from these pools. The weather had during the summer been unusually dry and favourable to such emanations. Accordingly I found that while only seven cases of fever had occurred in twenty-two houses, forming the longer of the two streets, 30 out of 50 (the entire number) had occurred in these twelve houses.

The proof was to my mind rendered complete by the immediate effect of the heavy rains which set in in July. The disease was stopped at once, and I have not heard of a case of fever in the same place since.

Similar proof, derived from the sanatory effect of the removal of the assigned cause, was afforded me in the case of a house in this county from which three servants were sent into the Navan hospital at short intervals labouring under continued fever, one of whom was also admitted a second time with severe dysentery. A very offensive smell had been long noticed in the yard adjoining the kitchen, and after the occurrence of these cases the sewer leading from the kitchen was in consequence examined, and found to be completely obstructed by a quantity of black putrescent matter. Upon the removal of this the smell of course disappeared, and no return of indisposition has since occurred, either among the servants or family. There was not the slightest evidence of contagion in any of these cases.

A reason for attributing the fever to the operation of endemial causes might be found in some instances, in the fact of the great indisposition of the disease to spread in the house in which a case occurs, even though the circumstances favourable to contagion may be present. Such an instance is given by Dr. Fergusson which we shall have again to notice.

In a paper on the statistics of fever, in Belfast, Dr. Mateer states that "one street, Carrick's Hill, and its continuation, Mill-field, with the adjoining lanes and entries, are found to have furnished three fifths of the whole amount of cases, and yet they are by no means the poorest or worst ventilated parts of the town." He attributes the prevalence of fever in this locality to the great want of water—"the consequences of which are want of cleanliness and bad sewerage, so that decayed animal and vegetable matter of all kinds, not being carried off by a current of water in the usual way, accumulate and generate miasmata."<sup>1</sup> This observation is of the more value that Dr. Mateer adduces it in support of the action of infection. Upon which an acute reviewer remarks:—"Surely this offers the very strongest argument against Dr. Mateer's own view of the extensive operation of contagion; why should this be all powerful in one particular locality? why should it not do its worst where poverty and bad ventilation flourish? why, but that on the large scale other causes of fever are far more potent than contagion."<sup>2</sup>

<sup>1</sup> Dublin Medical Journal, September, 1836.

<sup>2</sup> Medico-Chirurgical Review, Oct. 1836.

Lastly, the following case by Dr. Currie illustrates this form of fever in several particulars:—

“The 30th regiment, as is usual with troops in Liverpool, was billeted in the town but paraded and mounted guard in the fort, situated north of the town and on the banks of the river. The general guard room had been used, previous to the arrival of the 30th, as a place of confinement for deserters; it was extremely close and dirty, and under it was a cellar, which in the winter had been full of water. *This water was now half evaporated and from the surface issued offensive exhalations.*

“In a dark, narrow, and unventilated cell off the guard room it was usual to confine such men as were sent to the guard for misbehaviour, and about the beginning of June, 1792, several men had been shut up in this place on account of drunkenness, and suffered to remain there twenty-four hours under the debility that succeeds intoxication. The typhus or jail fever made its appearance in two of these men about the first of the month, and spread with great rapidity. Ten of the soldiers labouring under this epidemic were received into the Liverpool Infirmary. . . . . The symptoms of the fever were very uniform, in every case there was more or less cough with mucous expectoration; in all those who had sustained the disease eight days and upwards there were petechiæ on the skin, in several there were occasional bleedings from the nose and streaks of blood in the expectoration. The debility was considerable from the first. . . . . Great pain in the head with stupor pervaded the whole, and in several instances there occurred a considerable degree of low delirium. . . . . Our next care was to stop the progress of the infection; with this view the guard house was at first attempted to be purified by washing and ventilation; the greatest part of its furniture having been burnt or thrown into the sea. All our precautions and exertions of this kind were, however, found to be ineffectual. The weather was at this time wet, and extremely cold for the season; the men on guard could not be prevailed on to remain in the open air, and, from passing the night in the infected guard room, several of the privates of the successive reliefs on the 10th, 11th, and 12th of the month, caught the infection. . . . . No means having been found effectual for the purification of the guard room it was shut up, and a temporary shed erected in its stead. Still the contagion proceeded on the morning of the 13th, three more having been added to the list of the infected. On that day, therefore, the whole regiment was drawn up at my request, and the men examined in their ranks: seventeen were found with symptoms of fever upon them. It was not difficult to distinguish them as they stood by their fellows. Their countenances were languid, their whole appearance dejected, and the admata of their eyes had a dull red suffusion. These men were carefully separated from the rest of the corps and immediately subjected to the cold affusion. . . . . These means were successful in arresting



the epidemic—after the 13th of June no person was attacked by it.”

It may seem presumptuous to offer an opinion differing from that expressed by the distinguished writer under whose observation these cases occurred, but we think there is every reason to question the existence of infection, and to regard them as of purely endemial origin. Let us consider them with reference to the circumstances unfavourable to the existence of contagion before enumerated.

1. *The class of persons affected.*—British soldiers in time of peace are not obnoxious to contagious fever. The fact is stated by Dr. Cheyne, that while fever of this kind prevailed in the street contiguous to the principal barrack in Dublin, in 1817, and among a class of persons with whom the soldiers commonly associate, they escaped, because “little under the influence of the predisposing causes of fever; for the pay of the soldier is ample, he is well clothed, well fed, well lodged, and well looked after, and all his wants in health as well as in sickness are provided for.”<sup>1</sup>

2. *The season of its occurrence* is another strong reason for considering this fever of an endemial kind. A contagious epidemic may live out the summer, but unless it is imported we should doubt its being generated at that season.

3. *The locality* was also unfavourable. Isolated as it was, an imported contagion was unlikely.

4. *The inefficacy of all the preventive measures*, short of removal from the locality, with the immediate cessation of the disease which followed this step, are strongly opposed to the idea of infection. In fact if it be admitted that the stage of maturation or crisis is the period of infection, an examination of the dates of these cases will show that in no one was the disease so far advanced as to have enabled the patient to communicate it to his comrades, supposing them (which is not at all probable) to have had access to the hospital. On the other hand the positive evidence in favour of malaria is clear and decisive. Several individuals were exposed to this source during the debility which succeeds intoxication, and slept in its immediate neighbourhood. They were attacked, and others in succession as they became exposed to the same source. The malarious spot is abandoned on the 12th, and no case is observed after the 13th. Hereafter we shall attempt to show that the symptoms of these cases were such as characterise not typhus but typhoid fever—especially the late appearance of petechiæ, the exudations of blood from the air-passages, and the form of disturbance of the sensorial functions.

The above are a few of the instances which might be brought forward to prove the occurrence of fever in situations and circumstances unfavourable to contagion, and not liable to the objection that *filth* has existed merely in fortuitous connection with *fever* and

<sup>1</sup> Dublin Hospital Reports, vol. ii.

*poverty.* It would not be difficult to draw from the published histories of infectious fever (so called) such a number of similar facts, as would render doubtful the justice of Christison's objection, that "as for the few instances remaining, where true primary fever appears to originate in one of the above causes, all that need be said farther is that for one instance where such fever follows such cause, a thousand instances occur where no effect of the kind ensues, and that, consequently, some more essential influence is probably brought into play, than what appears merely on the surface of the investigation." But some argue that the disease produced is not fever—for, first, it does not diffuse itself as fever does by infection. This is not the place to enter upon an examination of the conflicting statements upon this question—we shall do so hereafter; but admitting that it has been asserted too hastily by Dr. S. Smith, and others, that *infectious fever* is generated by paludal sources, we deny that this justifies the inference sought to be deduced, that, therefore, *continued fever* is not so generated. On the contrary, it seems more consonant with reason to infer, that if fever affects a number of individuals in a certain locality without appearing to be communicated from one individual to another, and without, in any instance, being carried from that locality, this fever must arise from some local source common to all the affected persons. And if that party are in extreme, who hold that fever of a contagious specific character is daily generated by common causes external to the human body, equally so are the opposite party who deny to these sources the power to cause fever, "not typhoid," while at the same time they are ready to admit the identity of their own "specific contagion" with a disease which, the most eminent observers maintain, is never contagious! It is surely more consistent with the doctrine of the speciality of typhus to let it stand alone, and to give a place to non-contagious continued fever, than to exclude the latter by a doubtful assimilation of typhus and typhoid fevers. Hereafter we shall attempt to show that the most recent science is in accordance with the practical observation of Grant, that "these fevers, 'typhus,' are generally contagious, which the common fevers are not, unless their nature is altered, and they are rendered malignant *by bad treatment*"—while we may see reason in the present state of society in our large cities, in the widely prevailing influence of crowding, poverty, non-ventilation, &c. and the consequently frequent and facile transition of *common* into *contagious* fever, why the most opposite conclusions are formed as to their origin and diffusion, and why it happens, as Christison truly remarks, that "the greater proportion of the discrepant doctrines of the present day as to the origin of fever are founded essentially upon the same great body of facts."

Again, by some it is urged that the disease produced by paludal emanations differs from continued fever in symptoms and in type. Thus Dr. Christison alleges that "few inquirers have taken sufficient pains to distinguish primary continued fever from irritative

gastric fever." This objection cannot be allowed to have much weight so long as the *primary* nature of typhoid fever is a matter of dispute. Upon the subject of the type Dr. Christison may be quoted against his party, for if, as he asserts, "The coast remittent fever of Africa and other tropical countries seems to differ little in its characters from synochus, with a rapid and early stage of typhoid depression,"<sup>1</sup> what becomes of the argument against the malarial origin of continued fevers from alleged differences in the nature of these and the intermittent and remittent fevers, also produced by malaria? Besides how can the exclusive contagionist answer the anti-contagionist who rests his doctrine on such facts as those adduced by Armstrong: "Shortly after I had published my third edition on typhus fever, in which I had strenuously maintained the doctrine of human contagion, I met with a case of intermittent fever; in a few days the fever became remittent, and in a few days more put on the continued character, and the patient died with all the most malignant symptoms?"<sup>2</sup> or how will he dispose of the assertion of Dr. Elliotson, that most cases of so called typhus fever are really remittent,<sup>3</sup> or explain the occurrence (already noticed) described by Marsh and Addison of well-defined ague, following on the subsidence of continued typhus or synochus? Was the ague also the effect of contagion? Or will the contagionist escape from the necessity of adopting so easy a solution of the difficulty as the supposition of different morbid poisons, generated at different periods in the same locality, by a simple denial of the fact, and an impeachment of the accuracy of the observers who have recorded it? "In Sydenham's time," says Dr. Hancock, "and even in that of Fothergill, the quotidian of spring became continued fever in summer, while the simple continued fever of summer often changed to a malignant type in autumn. These were simple observations at a time when systematic arrangements had not put physicians in trammels. But now lest we should be guilty of medical heresy we must not insinuate that ague can change into con-

<sup>1</sup> Library of Medicine.

<sup>2</sup> Lectures by Rix.

<sup>3</sup> Lectures by Rogers. Dr. Mateer also observes—"We have the paroxysms of which fever is made up best seen in the intermittent and remittent fevers, but still by careful observation we can detect something of the same kind, though masked and often difficult to recognise, in the continued fevers of this country. These almost always assume more or less of the remittent character."—*Dublin Journal*, *ut supra*.

Dr. Currie remarks, whoever has watched the progress of fever must have observed the justness of the observations made by Cullen, Vogel, De Haen, and others, that even those genera which are denominated continued are not strictly such, but have pretty regular and distinct exacerbations and remissions in each diurnal period—*Med. Reports*, p. 16.

And Dr. Fordyce says, the similarity between these three kinds has determined practitioners of the greatest eminence, through the whole history of medicine, to consider them as the same disease. Many have thought that in a continued fever the subsequent paroxysm takes place in the hot fit of the prior paroxysm, &c.—(*Third Dissertation*, p. 59.)



tinued fever, and non-contagious fever into contagious typhus, either in an individual case or in the course of the year."

SECT. III.—*Varieties of the Sources and Modes of Application of the Poison.*—The organic matter constituting the source of the morbid poison may be purely animal, vegetable, or a mixture of both.

It must be admitted that fever seems to be very rarely produced by exposure to purely animal exhalations, and the facts brought forward by Bancroft, Chisholm, Duchatelet, and others, show that in the great majority of cases this exposure has been continued for any length of time with perfect impunity, but still there have occurred well authenticated instances to the contrary, some of which have been referred to; and a fact lately published, by M. Devergie, deserves farther notice. It is the occurrence of hospital gangrene in the hospital of St. Louis, which he attributes to the emanations from Montfaucon, since the disease was confined to the wards which were exposed to those emanations, and did not appear in other parts of the building.

Now if we admit the inference which seems naturally to follow from such instances as those related by Pringle, Hennen, &c., of the occurrence of typhus in the unwounded, in wards in which hospital gangrene existed, and of typhus attacking the attendants employed in washing the bandages of the same—namely, that hospital gangrene is a modification, or as it has been expressed, "a visible personification" of the typhus poison; we cannot avoid the admission that a fever poison may be generated by decomposing animal matter under certain conditions.

What the conditions required for this result may be, and why it so seldom happens that fever is thus produced, are questions to be resolved by deeper and more accurate investigations than appear yet to have been made.

There seems to be a more general belief in the activity of the vegetable poison, though why it might be difficult to say, unless from juxtaposition with the known fact of their power to cause periodic fevers; since there is at least an equal paucity of strict evidence with regard to this as to the animal source. About fifteen years since I witnessed the origin of a highly typhoid petechial fever in a healthy village in England, which appeared to arise from a vegetable source, a heap of putrefying turnips. In a house close to the nuisance, a boy had for two or three weeks been complaining of headache, lassitude, and debility, but had not been placed under any medical care. On the day on which I saw him he had been attacked with epistaxis which continued till his death, on the day following. His skin was covered with small ecchymotic petechiæ. After his death petechial fever appeared in the family, consisting of six persons, and in the adjoining houses, and proved fatal in several instances. It did not spread beyond the locality, and subsided in a few weeks. The season of the year (summer,) with

the other circumstances, were unfavourable to the supposition of contagion.

But it is to sources containing *mixed* organic matters that the experience of all observers point as most efficient in producing continued fever; such are slaughterhouses, obstructed sewers, cess-pools, &c. &c. It were needless to add to the details already given of cases originating in these sources.

The modes in which the poison may be applied to the organism are by direct introduction into the circulation, by being taken into the stomach, by inhalation, and by the skin.

Fever, or a disease confessedly bearing a close resemblance to it, has been produced in the lower animals by experiments too well known to need us to dwell upon them, and occasionally the typhoid symptoms which appear in other diseases would seem to be owing to the absorption of putrid matters into the circulation. A case of the kind once occurred under my own observation: a boy aged ten years was received into hospital, labouring apparently under typhoid fever; he sunk after a few days, and on dissection the only lesion discoverable was a carious state of the petrous bone, with a minute opening communicating with the lateral sinus, through which the matter of the carious abscess had passed into the circulation.

It is but rarely that an instance occurs of fever produced by putrid matters taken into the stomach, and the immunity enjoyed by savages, who live much upon putrid flesh, &c., has been referred to by Bancroft and others, not only against the fact, but also against the supposition of putrid animal matters containing a fever poison. It is not difficult to understand that this should be the case, since digestion, an antiseptic process, precedes assimilation, and changes remarkably the matters submitted to its operation; or, as accurately expressed by Liebig, putrid poisons having an *alkaline* reaction are rendered inert by the acids contained in the stomach, while these exert no such power over poisonous sausages which have an *acid* reaction. But this rule is not without some exceptions. In Dr. Christison's work on poisons is a report of a case which occurred in Stockport in the year 1830, of a family of five persons who were poisoned with broth made of putrid beef; in three instances the disease produced was severe, and in one fatal. It is worthy of remark (and is in accordance with the mode of action of a *morbid poison*) that in the worst cases the illness did not commence till the second (and in the fatal case the third) day after the meal. A case is somewhere narrated of a regiment in which putrid fever prevailed, and in which the disease was checked upon a discovery being made that the water used for drinking was drawn from a well in which some bodies were lying in a state of putrefaction.

Dr. Copland's remarks upon the effects of drinking the water of the Seine at Paris, and Dr. Hancock observes, that it has been fre-

<sup>1</sup> Dictionary of Practical Medicine, art. Endemic Influences.

quently remarked that this water produces diarrhœa in every one except the Parisian accustomed to the use of it.' Dr. Tweedie refers to the history of a fever ascribed to the combined effect of drinking putrid water and the emanations from the same—and others ascribe the putrid fevers of Paris to the fact that "there are numerous wells in that city from which many of the inhabitants derive their whole supply of water, not a few of which are situated in the very neighbourhood where the 'fosses' are the worst constructed and the least attended to; the urine, therefore, permeating the soil must necessarily contaminate the springs from which these wells are fed."<sup>2</sup>

The evidence given before the committee on the health of towns, by Mr. J. B. Wood, bears upon this question. After stating (qs. 2150-4) that 31,000 persons live in the cellars of Liverpool—forming two thirds of the working population—he states that, "in the districts in which these cellars are situated, there is a great deal of broken ground in which there are pits; the water accumulates in these pits, and of course at the fall of the year there is a good deal of water in them, in which there have been thrown dead dogs and cats and a great many offensive articles. 'This water is nevertheless used for culinary purposes. I could not believe this at first, I thought it was only used for washing, but I found it was used by the poorer inhabitants for culinary purposes.'"

The change produced in the fluids and solids of an animal by over driving seems to be capable of becoming a cause of disease in the human body. In the remarkable case given by Andral from Du Hamel it does not appear that fever, strictly speaking, was produced. The effect rather resembled hospital gangrene; but an instance is recorded of typhus fever following from eating the flesh of animals under similar circumstances. It is thus quoted by Dr. Gross:<sup>3</sup> "A few years ago a number of fattened cattle were driven into one of the New England cities, and having been pressed too hard in a sultry day were so overheated that some of them became quite exhausted. In this condition they were slaughtered, and the consequence was, as is stated by the reporter of the case, Dr. Fountain, that nearly all who partook of their flesh were seized with typhous fever."

These and similar observations would seem to show that the morbid poison, the product of putrefactive decomposition, may be received into the system through the stomach, more especially if presented in the fluid form; but there is every reason to conclude that fever is but seldom produced in this way, and that the general mode of introduction is through the respiration of the gaseous exhalations, which, as we have seen, are found upon their being collected and condensed to contain animalised matter in the state of

<sup>1</sup> Cyclopædia of Practical Medicine, art. Endemic Diseases.

<sup>2</sup> Medico-Chirurg. Review, vol. vi.

<sup>3</sup> Pathological Anatomy, vol. i. p. 223.



progress to decay, whose power of producing disease, in those exposed to their influence, has been questioned and is by many denied, but appears to be proved by evidence of a very satisfactory nature, and which applies with most force to cases occurring under circumstances unfavourable to the action of contagion.

SECT. IV.—*On the Mode of Action of the Poison.*—This does not appear to be attributable to its chemical qualities, but to its *condition*. It is the power of communicating an action, since there can be no doubt that its effects continue to be produced equally after the removal as during the presence of the cause, when once this cause has impressed its mode of action upon the organism. What the mode of this impression is, and what part of the system is the subject of it in the first instance, we have now to enquire, as also into the order of the phenomena subsequently produced and constituting the formed disease.

The generally received opinion seems to be that the nervous system is the subject of the first morbid impression, and its derangements the first in the morbid series constituting fever.

This doctrine is thus maintained by Dr. Southwood Smith<sup>1</sup>—“The immediate exciting cause of fever is a poison which operates primarily and specifically upon the brain and spinal chord. The diseased state into which these organs are brought by the operation of the poison, deprives them of the power of communicating to the system that supply of stimulus, (nervous and sensorial influence,) which is requisite to maintain the functions of the economy in the state of health. The organs, the seats of the functions, deprived of their supply of nervous influence, become deranged, the derangement in each taking place in a fixed order and in a determinate manner.

“Subsequently to the nervous and the sensorial, the organs the next to suffer are those of the circulation, then those of respiration, and, ultimately, those which belong to secretion and excretion. The condition of the nervous system, which produces this derangement in this circle of organs, occasions further, in that portion of the circulating system which consists of the capillary blood-vessels, that peculiar state which constitutes inflammation: hence inflammation is almost always established in one or more of the organs comprehended in the febrile circle and sometimes in all of them.” In another passage the same writer says—“The more closely and extensively the subject is investigated the more clear and satisfactory the evidence becomes, that the great primary cause of fever is a poison, the operation of which, like that of some other poisons, the nature of which is better understood and the action of which has been more completely examined, is ascertained to be upon the nervous system. How these poisons act upon the nervous system we do not know, nor can we possibly know as long as we remain

<sup>1</sup> Treatise on Fever.

so profoundly ignorant of the nature of the action of the nervous system in the state of health."

It will be seen that Dr. Smith's argument, like that of others already examined, is rested upon a fallacious analogy to certain other poisons, and like these is open to the fatal objection, that such poisons really produce their effect upon the nervous system subsequently to their diffusion through the blood. It moreover lacks the support derived in the former cases from the occurrence of a nervous shock, since such rarely, if ever, attends exposure to the paludal sources of fever. On the other hand, some of the arguments for a modified humoral theory before adduced, apply with greater force to this than the animal miasm, and additional ones are not wanting to strengthen the proofs that all derangements of function in fever are subsequent to the introduction of the poison into the blood, and the consequent vitiation of that fluid. For, its latency is even more remarkable than that of the animal infection of typhus, while a close observation of the state of the patient, during this period, will show a derangement of the secreting and excreting functions, which seem to be labouring to rid the system of the poison, or of those products which it has a tendency to generate in the blood; again, we can not unfrequently trace the abrupt termination of this period, and the supervention of formed disease to a suspension of the depuratory action, by cold, intemperance, or any other cause which disturbs the order of the excreting functions and arrests the elimination of the morbid product. Thus, in an exquisitely marked case of paludal fever, which was lately under my care, and which terminated fatally after large evacuations of blood from the bowels, it was remarked spontaneously by the patient's friends that, for six weeks before fever commenced, not only had he suffered from capricious appetite and irregular bowels, but that a remarkable thick copious deposit had been constantly present in his urine. During this period he had been living and sleeping in immediate proximity to a collection of filth, which at times filled the house (in other respects a cleanly and comfortable one) with an insufferable putrid effluvia.

This instance also illustrates the cumulative property of the poison, which is much more remarkable than that of the animal infection. Unlike the latter, which is often most severe upon its first introduction into a family, the poison of malaria seems commonly to affect each successive patient more severely than the preceding.

To what can this be attributed but to the accumulation in the blood consequent upon longer continued imbibition of the poison? This is similar to the explanation offered by Cruveilhier, of the general incurability of phlebitis from the absorption of pus. Experiments have shown, that from a single injection of putrid pus into the vein, an animal may, after copious evacuations, recover; he therefore concludes that similar success might attend the evacuant treatment of phlebitis, did not the renewal of the sources of

infection follow the incessant renewal of the pus. But we can not only detect the presence and agency of the poison in the circulation through the deranged excretions, but also occasionally in the physical changes of the blood itself previous to the occurrence of formed disease. The following observations of Dr. Potter<sup>1</sup> on this subject, are highly interesting and important, and from the care and accuracy with which they appear to have been made, are entitled to great weight in the discussion of the mode of access of fevers, arising from malaria. He says, "it was remarkable in all cases in which it was deemed expedient to bleed, the blood wore the same general appearances. After a separation had taken place, the serum assumed a yellow shade: often a deep orange, and a portion of the red globules was invariably precipitated.

"It occurred to me that if the remote cause resided in a common atmosphere, the blood of all who had inhaled it a certain time would exhibit similar phenomena. It accorded with the pathology I had conceived, to conclude, that all who lived in an atmosphere so impregnated were constantly predisposed, and that an additional or exciting cause only would be required to develop the symptoms and form. To ascertain the appearances of the blood in subjects apparently in good health, I drew it from five persons who had lived during the whole season in the most infected parts of the city, and who were, to every external appearance and inward feeling, in perfect health. The appearance of the blood could not be distinguished from that of those who laboured under the most inveterate grades of the disease. As this experiment might have been considered inconclusive unless the blood could be compared with that of those who lived in a purer atmosphere, remote from the evolution of miasmata, I selected an equal number of persons who lived on the hills of Baltimore County, and drew from them ten ounces of blood. The contrast in the appearances was so manifest that no cause for hesitation remained. There was neither a preternaturally yellow serum, nor a red precipitate; the appearances were such as we find in the blood of healthy subjects. A young gentleman, having returned from the western part of Pennsylvania on the 10th of September, I drew a few ounces of blood from a vein on that day; it discovered no deviation from that of other healthy persons. He remained in my family till the 26th of the month, and on that day I repeated the bloodletting. The serum had assumed a deep yellow hue, and a copious precipitate of red globules had fallen to the bottom of the receiving vessel. Of the six persons whose blood assumed those indications of the remote cause, four were seized with fever during the epidemic; the other

<sup>1</sup> Quoted by Dr. Tweedie, Art. Fever, Cyclopædia of Practical Medicine.

Dr. Tweedie's own expressed opinion is "whether the opinion of the older writers, that in fevers originating from contagion, the contagious principle alters the properties of the blood be correct or not, we certainly think the strong analogy in the cases alluded to tends to confirm the supposition of typhoid fevers originating in diseased blood."



two escaped any formal attack, but complained occasionally of headache, nausea, and other indications of disease."

Similar experiments were instituted by Dr. Stevens, and with a like result, the blood being black in colour, and evidently deranged in its properties previous to the commencement of the fever.

It might almost be considered unnecessary to strengthen the inference from such facts as these, by observing on the effect upon the blood of the gases, evolved from sources which we consider the evidence already adduced proves to contain a fever poison, when these are presented in a sufficient degree of concentration to produce rapidly fatal effects.

Describing the consequences of exposure to the emanations from Parisian privies, Dr. Christison says:—"The appearances in the bodies of persons killed by these emanations are *fluidity and blackness of the blood*, a dark tint of all the internal vascular organs, annihilation of the contractility of the muscles, more or less redness of the bronchial tubes and *secretion of brown mucus there as well as in the nostrils*, gorging of the lungs, an odour throughout the whole viscera, like that of decayed fish, and a tendency to early putrefaction."

With these and similar facts before us, we cannot agree in the sweeping decision of Dr. Smith, that "changes in the fluids can only be second in the series of morbid events: they can never hold the first place in that series: they can never be antecedents or first causes, but merely sequents or effects."<sup>1</sup> We rather think that the evidence existing on the subject, if fairly examined, points to the blood as the seat of the primary operations of the morbid agent, and the subject of the changes which it is a part of its condition necessarily to produce. And regarding all derangements of the functions occurring in fever as the consequences of the molecular changes in that fluid, we proceed to examine into the order in which these consecutive phenomena occur—the mode of their production, and their mutual dependence one upon another.

Supposing Dr. Smith's to be a fair exposition of the views generally entertained in this country, we find, upon reverting to his chapter on the theory of fever, that the doctrine maintained is, that subsequently to the supposed primary nervous impression, "the organs the next to suffer are those of the circulation, then those of respiration; and ultimately those which belong to secretion and excretion. The condition of the nervous system which produces this derangement in this circle of organs, occasions further in that portion of the circulating system which consists of the capillary bloodvessels, that peculiar state which constitutes inflammation: hence inflammation is almost always established in one or more of the organs comprehended in the febrile circle and sometimes in all of them." If this passage admits of a precise construction it must be that in consequence of a certain impression on the nervous

<sup>1</sup> On Fever, page 330.

system a state of general inflammation exists (or at least a state approaching to inflammation) in the capillaries of all the organs, and which is equally likely to become actual inflammation in any of them during fever. Without denying this frequency of visceral inflammation in fever, or the great necessity of recognising and combating it, it may be reasonably doubted if so variable and non-essential an occurrence—with one exception—or one so dependent for its existence and its seat upon accidental causes—as season, atmosphere, epidemic influences, states of constitution, &c.—as these local inflammations are, can be properly admitted into the discussion of a theory of fever. Either this combination of inflammation in some organ, with a peculiar state of the nervous system, is necessary to constitute fever, or it is not. That it is, seems unlikely, since morbid anatomy fails to detect it in a large proportion of instances. If it be not, then, according to this theory, nothing will remain but a certain peculiar affection of the nervous system to account for the phenomena of fever. But in the kind of fever under consideration there is a local affection generally regarded as inflammatory, and which is so constantly present and found to exercise so great an influence on the disease as to have been considered by some eminent pathologists to be the essential cause of typhoid fever. It is that affection of the mucous glands of the small intestines described under the name of dothinerteritis. This affection claims a special consideration, since no one can impartially examine the evidence put forward in support of their views of the pathology of fever by Louis and Chomel, in France, or the cases incidentally published in the writings of Bright, Tweedie, Smith, Graves and Stokes, Hodgkin, &c. in this country, without being convinced that in some forms of fever—which farther examination will show to be paludal fever—this dothinerterite is a constant and a most important complication, if indeed it be not the pathological cause of the disease.

But if we seek an explanation of its occurrence in Dr. Smith's theory, we are first at a loss to know why an impression on the brain and spinal chord should lead to consequent inflammation in a part the most remote of any from their influence, and whose functions are under the control of a different portion of the nervous system. Then, in very many cases, we find that the symptoms of intestinal irritation preceded this nervous impression—that in others, in which death took place at an early period of fever, it was found far advanced—as by Louis so early as the eighth day—and could not therefore be regarded as a secondary phenomenon. And that in other cases, in which the poison was presented in so concentrated a form that death took place before fever could be established, the glands of Peyer exhibited the same appearance as in that disease. The following interesting case of this kind is given by Dr. Christison.

In *August* last, twenty-two boys, living at a boarding-school at Clapham, were seized in the course of three or four hours with

alarming symptoms of violent irritation in the stomach and bowels, subsultus of the muscles of the arms, and excessive prostration of strength. Another had been similarly attacked three days before. This child died in twenty-five, and one of the others in twenty-three hours. On examination after death, the Peyerian glands of the intestines were found in the former case enlarged, and, as it were, tuberculated: in the other, there were also ulcers of the mucous coat of the small intestines, and softening of that coat in the colon. A suspicion of poisoning having naturally arisen, the various utensils and articles of food used by the family were examined, but without success. And the only circumstance which appeared to explain the accident was, that two days before the first child took ill, a foul cesspool had been opened, and the materials diffused over a garden adjoining the children's play ground. This was considered a sufficient cause of the disease, by Dr. Spurgin and Messrs. Angus and Saunders of Clapham, as well as by Drs. Latham and Chambers and Mr. Pearson, of London, who personally examined the whole particulars." There cannot, we think, be a doubt that their opinion was correct, and that nothing but the rapid termination prevented the developement of the phenomena of fever in these cases; but in fairness to Dr. Christison, it should be added, that he considers "this opinion cannot be received with confidence by the medical jurist and the physician, since it is not supported by any previous account of the effects of sulphuretted hydrogen." Perhaps these cases may receive some confirmation from the following report (certainly not a full one) of a similar accident by Dr. Arnott.<sup>1</sup> "In a mews behind Bedford-square, a stable had been let for a time to a butcher, and a heap of dung had been formed at the door, containing pigs' offal, pigeons' dung, &c. During the act of removing this heap, a coachman's wife and her three children, of an adjoining stable, sat for a time at an open window nearly over the place, until the insufferable stench drove them away; two of the poor children died of the poison before thirty-six hours, and the mother and other child narrowly escaped."

In Dr. Christison's cases the description of the appearances of Peyer's glands exactly corresponds (especially the first case) with Dr. Bright's description, and with the representation given in one of the plates in his great work. As also with the minute investigations of Dr. Staberoh, who regards the first stage of the follicular affection, as "an infiltration into the mucous coat, and especially the crypts called Peyerian glands," but also taking place, as he has repeatedly seen it, in different parts of the colon, "and to which he considers the inflammation of the mucous membrane secondary."—(Dublin Journal, vol. 13.)

But farther—if dothin-enterite were a consequence of disordered circulation depending upon an impression on the brain and spinal chord, we might expect to meet it in other cases in which these

<sup>1</sup> Fourth Report of Poor Law Commissioners, p. 106.



organs are engaged, as in the periodic fevers and in typhus: but we do not, nor can any other local inflammation be named as similarly constant in these diseases, and filling its place in the "febrile circle." On the contrary, M'Cartney, Armstrong, and others, have fully proved that the vascular congestion commonly found in these diseases is not of an inflammatory nature, and that though it may remotely give rise in certain cases to an inflammatory reaction for its removal, it is yet a distinct pathological condition.<sup>1</sup>

Two other opinions may be entertained of the relation of dothinitis to fever. One, that it is the primary cause: that fever is the sum of the symptoms of this inflammation—the other, that it is the specific effect of the septic poison from which typhoid fever originates, and like the other symptoms of this disease, merely a link in the chain of sequences constituting fever.

Perhaps the strongest arguments for the first of these opinions are the large proportion of cases of typhoid fever in which it is found to exist—the influence it exercises upon the severity of the disease, and the effects of antiphlogistic remedies, more especially of topical bloodletting. It will be presently seen that the two first circumstances are equally well explained upon the second opinion. With reference to the effects of bloodletting it must be admitted that a very considerable amelioration of symptoms, and not unfrequently their total removal, has followed timely and free abstraction of blood, especially by leeches applied over the affected intestine, and not only are the tenderness and pain in the part, with the meteorism and diarrhœa thus relieved, but the headache, thirst, pulse, and other general symptoms commonly undergo at least a temporary and partial improvement. But it may be doubted whether this is to be attributed so much to the removal of inflammation as to an impression made upon the general disease, by the new movement given to the circulation in general by the smallest local abstraction of blood, and which is felt in every part of the system.

One or two circumstances may be cited in proof of this dynamic effect of bleeding, and to illustrate its application to the present case—1st, the well known fact that the impression made upon the central organ of the circulation by the bleeding from a few leech bites is totally disproportionate to the quantity taken away. 2d. The effect of leeching, or bleeding in some other disorders in which no supposition of inflammation could exist. In amenorrhœa, for instance, we have known the disorder of several months' standing removed by the application of a few leeches to the inguinal region before the leeches were themselves removed. In ague also, an effect almost equally marked may be sometimes produced by the same means. The following short case illustrates this. C. F—, æt. 20, was admitted into the Navan Hospital on the 22d of February, labouring under tertian ague. The fit comes on two hours

<sup>1</sup> For Dr. M'Cartney's observations, see *Dub. Med. Trans.* vol. ii. p. 574.

earlier at each period. Has some tenderness on pressure, and fulness of epigastrium, thirst, tongue red at tip and edges—previous to entering hospital took two emetics and an aperient. A fit took place about 3 A. M. on the 21st, and might be expected to recur at 1 A. M. on the 23d. I ordered 12 leeches to be applied on the evening of the 22d to the epigastrium, and a draught to be taken at bed time, containing twenty drops of laudanum. The fit occurred at 5 A. M., four hours later than it was expected. On the 25th it came on at seven, and the leeching being repeated on the evening of the 26th, he sweated copiously during the night, and had no return of the fit. There was a slight return of it five days after, but from this time he got rapidly well.

Without entering farther into the discussion of the theory, that dothinen-eritis is *the cause* of the febrile phenomena, we pass on to submit certain considerations in support of the view which regards it as an effect not *of* the fever or of the state of the nervous and circulating systems produced by the fever, but as the direct effect of the poison itself—as one (probably the first) of the links in the chain of sequences, constituting fever, and one upon the occurrence of which some of the others may probably depend. This view approaches nearer to that of Louis, who is of opinion that the affection of the follicles occurs in the beginning of the disease, than to that of Chomel, who seems inclined to admit its classification among the secondary inflammations, but differs from the definition of the former eminent pathologist, inasmuch as it seeks to establish the agency of a morbid poison as *the cause of fever*, in place of his decision “that *the cause is unknown*.” The following extract from his summary of the diagnostic symptoms of typhoid fever is important, as containing two particulars which we shall find to have a bearing upon this inquiry.

“*Maladie aiguë accompagnée d'un mouvement fébrile plus ou moins intense, variable dans sa durée; propre aux jeunes sujets, principalement à ceux qui se trouvent depuis peu de temps au milieu de circonstances nouvelles pour eux, dont la cause est inconnue; debutant par un frisson violent, l'anorexia, la soif, et dans la très grande majorité des cas par des coliques et la diarrhée,*” &c.

The two circumstances here mentioned by Louis, of the subjects of typhoid fever being those newly exposed to influences, the nature of which he concludes are not known—and the diarrhœa which ushers in the complaint—we conceive tend to support the theory that the intestinal affection is a consequence of the effort made by the excreting organs—more especially the liver—to rid the system of the poison which has been introduced into the blood. It is easy to conceive that the native of Paris, born and brought up in the atmosphere of its fosses, and drinking all his life the tainted waters of its wells and river, may habitually eliminate from the blood such products as are thus taken in unfit for assimilation or nutriment, and that to a constitution unused to such a task the consequence of taking into the circulation the same decomposing

substances should be different. And if we aid our conception of this fact by a reference to what takes place in the different classes of persons exposed to malaria of other kinds in temperate climates, we shall see why intestinal affections should be among the first consequences of the process; for it will appear that the liver is the organ by means of whose excretions the poison is attempted to be got rid of, and according to the facility with which this is performed or otherwise, will be the chances of escape or the contrary, from the effects of the poison. It is well known that, while the lungs of a native of a warm climate are liable to become diseased upon removal to a cold one, the liver is the organ prone to suffer upon the inhabitant of a cold removing to a warm climate. It is also found that the stranger from a colder country will rapidly contract fever from exposure to malaria in a temperate climate, while the person newly arrived from a warm one will not be similarly affected until that change in the order of his functions termed acclimatization has taken place, and he becomes assimilated in habits to the inhabitant of the same latitude.

The following are the data upon which the proof of the connection of diseased mucous follicles with the peculiar effects of a morbid poison upon the biliary excretion, may be rested.

1st. It may be considered as admitted, that the special characters of substances fitted for assimilation are absence of active chemical qualities, and the capability of yielding to transformations; and that every substance may be considered as nutriment, which loses its former properties when acted on by the vital principle, and does not exercise a chemical action upon the living organ.

2dly. That in the progress of the functions of nutrition, certain chemical and organic substances are produced, and from time to time are present in the blood; which products it is the office of the different excreting organs to discharge from that fluid—the relative activity of these organs depending partly upon the matter to be eliminated, and partly on other circumstances; thus we have seen that, in one situation the lungs assume a disproportionate activity, in others, the liver, &c.

The foregoing propositions being admitted, the following may be regarded as convertible from them. If substances be introduced into the blood which are *not* capable of assimilation or of affording nutriment—whether from their chemical qualities or from their condition (of decomposition)—it will follow that, instead of these suffering the transformations which food undergoes to become assimilated, the blood will undergo their transformation and disease will be produced.

Also, that numerous modifications in the composition and condition of the compounds, produced from the elements of the blood, may be the immediate result of the introduction into it of these substances, and a change in the quality of the excretions may thus be the first indication of the action of the poison, as well as of the effort made to expel it.



Numerous facts and observations tend to show that, in the case of organic or putrid poisons, the *liver* is the organ by whose excretions an attempt is made to rid the blood of the new products thus formed in it.

As first—by a reference to the experiments of injection of putrid pus, &c. into the veins of animals, performed by Magendie, Gaspard, Cruveilhier, &c. it will be seen that when the animal recovered it was after copious discharges of a vitiated character from the bowels; to these discharges the last named writer attributes the recovery, and adds, that it is a fundamental fact of pathology that the intestinal canal is chiefly affected in diseases caused by miasmata.

Again, if we refer to the published cases of poisoning from putrid ingesta, we see that, besides those of irritant poisoning in which the rapid rejection of the substances was followed by recovery, there is another class in which, after an interval allowing of the absorption of the poison into the circulation, a different set of symptoms followed, as in the following from Dr. Christison's work on poisons:—"A family of five persons took for dinner broth made of beef, which owing to its black colour the master of the family had previously said to his wife he thought bad and unfit for use.

"In the course of some hours two boys were attacked with sickness and vomiting, but appear to have got soon well, probably from the early discharge of the poison. Next morning a washer-woman, who had dined with the family, was seized with violent *pain in the bowels, diarrhœa, racking pains, and weakness in the limbs*, and did not recover for ten days. On the evening of the second day the master of the house was similarly affected and was ill for a fortnight. And a day later, his wife was also seized with a similar disorder, preceded by soreness of the throat and tongue, and difficulty of swallowing, and ending fatally in fourteen days."

It is worthy of notice that the severity of these cases was in proportion to the interval allowed for absorption of the poison—together their resemblance to the description of the symptoms of typhoid fever, quoted from Louis, is remarkable. If we inquire why the mucous glands of the lower portion of the ileum, are more than other parts of the intestine liable to suffer from this peculiar derangement of the biliary excretion, we shall see reason to think the cause is the same as would explain their existence in greater number there than elsewhere, and that this is probably owing to the fact of a second digestion or chymification being performed in the cæcum, during which, it is believed by some physiologists,<sup>1</sup> that the entrance to the large intestine is closed, and bile collected in the lower portion of the small intestine, which does not enter the cæcum till the secondary chymification is completed. The effect of such a retention of an acrid and depraved secretion must be obviously to produce irritation in the part subjected to its influence,

<sup>1</sup> Schultz quoted by Müller.

and the same deranged products of secretion, continuing through much of the duration of the fever, we can account for this affection not seeming to be limited to any portion of that period, but why commencing with it—frequently even preceding it, it ordinarily survives the continuance of the most prolonged disease.

This explanation also accounts for the disease in these glands being found farthest advanced nearest to the termination of the intestine—for perforation occurring almost invariably close to the cæcum—for the lymphatic glands of the mesentery, corresponding to the diseased follicles, becoming diseased—and for the severity of these affections bearing a direct proportion to the severity of the fever, and, as it would appear, to the amount of the poison imbibed into the system.

This view of the relation of diseased follicles to the action of the septic poison differs both from that which regards dothineritis as the cause of fever, and from that which assigns to it a merely secondary place in this affection. To the latter are opposed the extremely frequent and early occurrence of abdominal symptoms (as diarrhœa) in the typhoid form of fever. With the post mortem appearances in subjects examined at an early stage; while the former is irreconcilable with the occasional absence of the lesion, the frequent want of correspondence between its amount and the gravity and fatal result of the fever, (a correspondence which should exist if the other phenomena of fever were but the sympathies of the affection of the follicles,) with the occasional persistence of the local disease after the fever has subsided, and with the presence (almost equally frequent in typhoid fever) of other lesions which cannot be considered sympathetic of this, but must be ascribed either to the immediate operation of the poison, or to that state of the blood produced by it; such are the softening of the spleen, liver and heart, and the inflammatory affection of the brain and thoracic viscera.

These pathological changes, as well as the derangements of function constituting the *febrile state*, will probably be best explained by some such hypothesis as that advanced by Dr. Hodgkin, which supposes the febrile state to depend upon a suspension, or at least very considerable interruption of that process by which, during health, the various parts of the system are continually undergoing a change, the old materials being removed while others are substituted in their place."<sup>1</sup> This hypothesis will be found perfectly in accordance with that of the action of a morbid poison

<sup>1</sup> Lectures on Mucous Membranes. Lect. 23.

Dr. Hodgkin's hypothesis seems to explain the great difference in the fatality of fever as affecting the higher and lower classes of society, since by the mode of life of the former, more nutriment being taken into the system, and more organic matters constantly present in the blood, an arrest of the process by which they are eliminated, must naturally be followed by more complete deterioration of the mass of circulating fluid, and more serious injury to the functions and structure of the organs supplied by it.

upon the blood, since it will be the natural effect of the molecular change produced in that fluid by the decomposing particles of the poison so to modify it as to render it unfit to undergo the capillary attractions constituting the processes of interstitial absorption, nutrition, and secretion; and thus instead of Dr. Smith's formula of the order of successive derangements in fever—namely, derangement of innervation—then of circulation, and lastly of secretions and of the animal fluids; the more correct one will probably be, first the molecular change in the blood, then the suspension or modification of the interstitial processes—or change of particles—then certain derangements of innervation and of the heart's action, and the result—formed fever.

According to this view, dothineritis is one of the phenomena of the second stage in the action of the poison, and immediately consequent upon certain modifications of the biliary (and probably also the intestinal) secretion. Its occurrence cannot be considered *essential* to typhoid fever, as the contamination of the blood may cause the molecular changes upon which the foregoing hypothesis supposes fever to depend without this—albeit its absence in typhoid fever is very rare—while, on the other hand, it may exist without fever necessarily following, for we frequently see that, of a number of individuals exposed to the same source of miasm, some will suffer an attack of typhoid fever, while others will be affected with diarrhœa or dysentery: a fact which is explained by a reference to those experiments of Gaspard, in which the recovery of the animal after putrid injection, was attended with profuse and offensive discharges—seemingly the mode of relieving the blood from the presence of the poison.

Reference has been already made to another set of cases, in which dothineritis occurs without fever: those namely, in which the poison was so concentrated as to produce a rapidly fatal effect, and where examination after a diarrhœa of only a day or two showed the same peculiar affection of the follicles as in typhoid fever; the inference from such cases taken conjointly with those of fever from the same causes without dothineritis, must be that this lesion is *neither cause nor effect of the fever, but a concurrent and contingent effect of the poison*. Most of the other pathological changes of fever are to be explained by the alterations in the constitution of the blood. Such is evidently the cause of the softening of the spleen so invariably present in typhoid fever, and such a little consideration will show to be the cause of the congestive character of the typhoid inflammations: for the occurrence most likely to follow such a change in the molecular attractions of the blood as will interfere with its capillary circulation, is *stagnation in this part of the system*, of which the consequences are venous congestion, passive hemorrhages, and the softening of parenchymatous organs. The stagnant character of the typhoid pneumonia has been remarked by many; thus Dr. Williams says, "it may be almost a



question whether in these cases the local disease in the lungs is not rather a congestion of blood in an altered state than an inflammation, and it is very commonly the sequel rather than the cause of the fever,"—an opinion which seems fully warranted by dissection, as well as consistent with the modifications of the physical signs in this form of disease.<sup>2</sup> Huxham was so much struck with this connection of the local affection and diseased blood, that he compared the state of the latter in these cases to the *scorbutic habit*; and Andral countenances this analogy in the following passages. "The ataxo-adyamic fever recognises for its commencement some alteration of the blood, whether this alteration may have taken place spontaneously, and produce a sort of acute scorbutus, or it may follow the introduction of deleterious agents, as miasms, virus, matters in a state of putrefaction; these agents after having modified the composition of the blood come to poison the nervous centres. Then the disease is everywhere, where blood and nerves are to be found, and in every part lesions may occur which perform but a secondary part in the production of the symptoms."<sup>3</sup>

*Again*—"Congestions of the parenchymatous tissues and membranes are tolerably frequent during the course of fevers. These congestions seem to depend on the rupture of equilibrium between the globules and fibrine; they are very frequent in typhoid and typhus fevers, and small-pox; the spleen and other parenchymatous tissues are usually congested in these diseases, and a diminution of fibrine as compared with the globules (whether absolute or relative) is the alteration of the blood observed in these maladies.

"The ancients concluded from the phenomena just mentioned that in the diseases in question, the blood is altered, and that its elements have a great tendency to separate. They designated by the phrase *putridity*, that morbid condition in which the vital powers seem to yield to physical causes, and the blood becomes putrescent. Borden, whose opinions as to the nature of typhoid fever are remarkably sagacious and philosophical, does not hesitate to consider that malady as connected with a general condition of the system, which he designates by the name of *acute scurvy*. This phrase is not inaccurate, so far as regards the condition of the blood. A diminution of the quantity of the coagulable material of the blood is a general fact observed in all great febrile disturbances; thus in miasmatic fevers there is first absorption of the miasma, and immediately after, the only prominent phenomenon is an al-

<sup>1</sup> Article Pneumonia, Cyclop. Prac. Med.

<sup>2</sup> See my Observations on Typhoid Pneumonia, Dublin Journal, vol. vii. for several dissections of this disease.

<sup>3</sup> Clinique Medicale Translated, p. 610. Several cases are given, in which all the symptoms of typhoid fever were produced apparently by mental and bodily depression, but after death no lesion was discovered—for a striking case of this acute scorbutus, see Dr. Law's paper before quoted.

teration of the blood. This alteration which occurs in typhoid fever is the effect of some cause as yet unknown.”<sup>1</sup>

Hemorrhage is well known to be characteristic of typhoid fever. That from the air passages is enumerated by Chomel among the distinctive symptoms of the disease. Intestinal hemorrhage is also a frequent and an unfavourable symptom, and indicative of a diseased state of the blood. This hemorrhage is preceded by stagnation. The softening of viscera is always observed in conjunction with an altered and fluid state of the blood. In some descriptions of softened spleen by Andral and others, the blood contained in it is compared to the lees of wine.

SECT. V.—*Characters of the disease produced by the putrid miasm.*—It has been remarked, that a general resemblance may be traced between the disease produced by the infectious animal poison of typhus and the exanthemata; its first and most striking analogy being the almost constant presence of a peculiar eruption. Another particular in which it resembles them and differs from the disease now under consideration is the absence of any constant internal lesion; the pathology of typhus being of a functional or physiological kind, while that of endemic or typhoid fever is anatomical and precise in its nature. The distinctive characters of the two affections may be thus stated. In typhus, a poison is generated by certain changes in the fluids of the living body, which, being received into the blood of a healthy individual, has a tendency to excite in that fluid the transformations from which it has itself arisen, and by which it will be reproduced; a process during which certain phenomena occur, as that of eruption (an effort apparently to free the system from the presence of the poison,) and the conclusion of which is marked by the presence in the excretions of the material necessary for the generation anew of the disease in any person into whose blood it may be received. All these phenomena may occur without appreciable change in the structure of any organ, and in fact death may be produced without any morbid appearance beyond that degree of congestion naturally connected with the modification of its processes of nutrition and secretion.

In typhoid fever the events following the introduction of the putrefactive poison are different; it will appear upon examination into these, that no new material of reproduction is generated, that the eruption is not a true exanthema or identical with that of typhus, being later in its appearance, less constant, more scanty, consisting of successive crops rather than persistent and uniform, as in typhus. A marked modification of the molecular changes of the system occurs in this as in every variety of febrile movement, but its continuance is evidently less uniform than in typhus, being subject to alternations and remissions, at times approaching those of the periodic fevers; and the critical change attending its reso-

<sup>1</sup> Lectures on the Blood, reported in Dublin Medical Press, Aug. 11, and Provincial Medical and Surgical Journal, Aug. 21.

lution is more gradual and liable to be less certain and complete, as well as to recur by relapse, unlike that of typhus. But the most important distinction consists in the fact that the typhoid miasm has, like other putrid poisons, a tendency to be eliminated from the system through the biliary excretion, in the course of which process a peculiar form of irritation is set up in the alimentary canal, while no such tendency can be asserted of the poison of true typhus, in the majority of cases of which the biliary excretion suffers rather a diminution than otherwise.

But it may be said it is by no means proved that the typhoid affection of Louis and Chomel is of endemial origin, and in order to establish the connection between miasm as a cause, and fever characterised by dothineritis as the effect, either this must be proved, or it must be made to appear that the fevers which in our own country may be traced to this source are to be distinguished from typhus by the intestinal lesion.

With regard to the French typhoid fever, we are led to infer this conclusion from the following facts: the existence of such miasm in abundance in the fosses, wells, and river of Paris—the almost invariable occurrence of gastro-intestinal affections in those newly-arrived there—the fact that typhoid fever attacks the same class so constantly as to make a change of circumstances regarded as one of the essential causes of the disease—and lastly, the testimony of the most distinguished physicians that it is not propagated by contagion. Let the experience of our large hospitals, with reference to the infection of typhus, be compared with the following statement of Andral. “In Paris, either in the hospitals or out of them, we never recognised in this disease (dothineritis) the slightest appearance of a contagious character. In the hospitals we do not see it transmitted from the individual who brings it from without to those who are lying in the beds next his own; neither do we see that the patients who lie in a bed previously occupied by a person who has recovered from, or who has died of a dothineritis, are attacked by it; neither are the physicians or medical students who come there attacked with it, more particularly those who have had to come in contact with patients labouring under the disease. Out of the hospitals what circumstances are more favourable to contagion than those generally found combined in the case of medical students who attend their companions when affected with typhoid fever? Shut up in a room which in general is very small, they pay them the most assiduous and devoted attention night and day; if the affection were contagious almost all of them would contract it, and yet we do not remember to have seen the disease even once arise in this way in a healthy individual.”<sup>1</sup>

Louis does not mention contagion in his observation on the causes of the disease, but Dr. Gerrhard states that, in conversation, he informed him that he had never seen a case so communicated.

<sup>1</sup> Spillan's Clinique Medicale, p. 728.



But the question may be elucidated by an examination of some of the published histories of fevers occurring in this country from exposure to endemial sources, from which it will be seen that in numerous instances these were found to be attended with the characteristic dothineritis of the French typhoid affection. Thus in London, after making every due allowance to the advocate of the exclusive infectiousness of typhus, we must contend that the writings of Dr. Armstrong, Dr. Southwood Smith, and others, prove the frequent occurrence of continued fever from these causes, while the treatise of Dr. Smith shows how large a proportion of the fever of London is of the intestinal kind—having all the characters, symptomatic and anatomical, of the “typhoid affection.” A similar remark may be made of Dr. Tweedie’s work, which contains numerous cases of dothineritis, while it affords strong indirect testimony to the endemial source in the statement as to the period of the year at which the disease prevailed, and its remarkable subsidence under the influence of low temperature, rain, and frost—causes which exert a precisely opposite effect upon contagious typhus.<sup>1</sup>

<sup>1</sup> Dr. Tweedie, who is by no means a strong advocate for the malarial origin of fever, remarks, that “cold and wet summers are always remarked to be comparatively healthy, while disorders of the bowels in such seasons are seldom observed. The number of patients admitted into the Fever Hospital, in the Autumn months of the last three years, establish this principle. In August, September, and October, 1827, there were 205; in the same months of 1828, the numbers were 170; in the Autumn of 1829, only 94 were received. The cause of this progressive diminution is undoubtedly to be traced to the cold wet summers of the last two seasons.”

A similar remark has been made by many physicians as to Dublin; thus Dr. Percival says, “it has long been observed, that protracted dry weather is peculiarly productive of fever in Dublin; and that rainy weather agrees best with the general health of its inhabitants.” And while he states that the worst forms of typhous fever prevailed at an advanced period of the *winter*, and were characterised by cerebral congestion, he thus distinctly characterises the endemial fever: “But the seat of peculiar congestion in the autumnal fever was the inner surface of the intestines, and sometimes the mesenteric organs. The type of this epidemic was more irregular than any other; its invasion more obscure; its progress and duration less defined. The subjects of the disease were often broken down and declining constitutions, in which the digestive organs had been long impaired, &c.” Could any description more resemble that of the dothineritis of Louis? Dr. Davidson, who argues for the identity of the two fevers, meets the above statement of Dr. Tweedie thus:—“An opinion exactly opposed to that of Dr. Tweedie is given by Dr. Armstrong. He states, that in England, typhus is evidently favoured by a low temperature, being most prevalent in the cold seasons of winter and spring, generally abating or disappearing, as the heat of summer advances, and often prevailing to a considerable degree in cold wet autumns.” This passage is extracted from his work on typhus, in which Dr. Armstrong advocated the doctrine of contagion. At a later period of his life he taught the exclusively malarial origin of fever, and in his lectures adduces in proof of that doctrine the great prevalence of fever in London during hot seasons, and particularly during the dry hot summer of 1818. The only mode of reconciling these opposite opinions of the *same*

Again, while Dr. Addison ascribes the numerous cases of synchus and typhus presented at Guy's Hospital to river malaria, Dr. Bright and Dr. Hodgkin prove the identity of many of the cases received into that institution with the disease described by Louis. Similar observations have been made in other places. In Birmingham, dothineritis is stated to be the constant morbid appearance of the few cases of fever which occur; and on referring to Dr. Ward's account, already quoted, of a fever which he clearly shows to have arisen from river malaria, we find it stated that it was present in all the fatal cases.

In Dublin, Dr. Cheyne marked three periods occurring in his experience, during which the contagious typhus, usually prevalent, gave way to epidemics of intestinal fever, in which evidence of malaria was frequently met with, but infection not so—the pathology was that of dothineritis. These observations are confirmed by others; thus, Drs. Graves and Stokes have published a number of cases of peritonitis from perforating ulcer of the ileum, occurring during one of these periods, 1826–29. Dr. Kennedy states that the glands of Peyer were found by him to be more or less diseased, in a large proportion of the cases of the same period, presenting, as he remarks, a striking contrast in this respect to the fever (contagious typhus) of 1837.<sup>1</sup> Dr. Stokes also says, “In the epidemic of 1826 and 1827, we observed the follicular ulcerations (dothineritis of the French) in the greater number of cases. In many instances perforation took place, and the whole group of vital and cadaveric phenomena corresponded almost exactly to the dothineritic affection of the French authors.”<sup>2</sup>

We meet with similar evidence of two fevers in Glasgow. In 1836, says Dr. Stewart, I was much struck with the simultaneous occurrence in the wards of the Glasgow Fever Hospital, of two sets of cases in which the symptoms (however little most of them might seem to differ when viewed individually) presented, when taken collectively, characters so marked as to defy misconception, and to enable the observer to form with the utmost precision the diagnosis of the nature of the disease and the lesions to be revealed by dissection. More particularly it was remarkable to observe, that while in the one disease the affection in those who presented no eruption was so slight and of so short duration as to make it very questionable whether it deserved the name of typhus, and while the fatal cases presented an abundant and generally a profuse eruption; those labouring under the other, which equally and even in a much higher proportion, went on to a fatal termination, rarely pre-

*observer*, is by supposing that he described the nature of each epidemic correctly as it was presented to him, but not being prepared to recognise a distinction in their nature, was naturally led by the evidently non-contagious character of the last observed, to doubt the correctness of his views of the origin of the first.

<sup>1</sup> Medical Report of the Cork Street Fever Hospital.

<sup>2</sup> Lectures, Lond. Med. and Surg. Journal.

sented any, and then only a very scanty eruption. It was further remarkable, that while in the one several successive patients had either been restored to health or fallen victims to the severity of the affection, the disease under which those laboured who lay side by side with them, though characterised by much less urgent symptoms, pursued its gradual course through weeks and months consecutively, and in the majority of cases to a fatal issue. And finally it was more remarkable still, that to complete the contrast already so striking, dissection proved the existence in the one disease of most extensive local lesions, in the other, the absence of all prominent local lesion whatsoever.

Dr. Stewart adds, "that during the summer and autumn of 1836, the cases of typhoid fever were numerous, but from the month of November in that year, (at which time both the type and amount of typhus became more formidable) till June, 1838, not more than a dozen cases, if there were even so many, and these at long intervals, were admitted for treatment."<sup>1</sup>

This evidence is pretty clear as to the existence of two forms of disease. As to the causes, the highest authority in Glasgow on this subject, Dr. Cowan, writing during the period referred to, and with the cases before him, says—"many of the cases of the production and propagation of disease must be ascribed to the habits of our population, to the total want of cleanliness among the lower order of the community, to the absence of ventilation in the more densely peopled districts, and to the accumulation for weeks and months together of filth of every description in our public and private dunghills, to the over crowded state of the lodging houses resorted to by the lowest classes, and many other circumstances unnecessary to mention."

In Edinburgh, according to Dr. Christison, "the intestinal affection has repeatedly presented itself in groups—the *constitutio dothinerterica*, to speak in nosographical language, has repeatedly appeared and disappeared as a subordinate or intercurrent epidemic, in the course of the more general epidemic—typhus." And according to Dr. Reid, such cases occur not unfrequently in the country parts of Scotland, and are occasionally sent to the Edinburgh Infirmary.

In Liverpool we are informed they occur in an intercurrent way, as in Glasgow,<sup>2</sup> and we need only refer to the evidence before the committee on the health of towns for proof of sufficient endemic causes.

In the Navan Fever Hospital there have been for the last seven years almost always two distinct forms of fever present, one or other occasionally preponderating, so as at times nearly to exclude the other. Thus for the first three years the prominent features were pain, tenderness, and meteorism of the abdomen, diarrhœa,

<sup>1</sup> On Typhus and Typhoid Fevers, Edin. Med. and Surg. Journ. No. 145.

<sup>2</sup> See Dr. Lombard's Letter, Dub. Med. Journal, vol. x.



and not unfrequently these symptoms combined with catarrh; several cases of perforation of the ileum occurred towards the close of this period; petechiæ were not frequent and were late in their appearance, and we had few instances of communication by contagion. During the three following years a highly contagious fever prevailed, and the symptoms and treatment were completely different, delirium, subsultus, dysphagia, being the ordinary symptoms, and diarrhœa being rarely met with;—nearly every case presented the measly efflorescence, and instances of contagion were as numerous as they had been rare previously. During the present summer the prevailing type has been the abdominal fever of the first period, and instances of typhus are infrequent, certainly not a fourth of the whole, and sent exclusively from a district in which the epidemic of last year still lingers.

In America the existence of two kinds of fever has been maintained by Dr. Jackson and Dr. Gerrhard. The former says, in his report on typhoid fever, "it is plain that there are at least two species of continued fever, both in Europe and in this country, and further researches may very possibly show more."

Dr. Gerrhard states, that "from the information we possess we should conjecture that the two diseases (British or Irish typhus and dothinenteritis,) are widely different in their symptoms, anatomical characters, treatment, and mode of transmission."

The following extracts from his able paper will show that the two forms of fever exist in America at different periods and with distinct characters, just as in our own cities.

"Dothinenteritis is by no means a rare disease in Philadelphia, although less common than at Paris. In the essay alluded to, I established the identity of the anatomical characters and of the symptoms of the fever occurring at Philadelphia with that observed at Paris. . . . The typhus fever which is so common throughout the British dominions, especially in Ireland, is not attended with ulceration or other lesion of the glands of Peyer. . . . For a period of at least ten years, there has been no epidemic of this nature at Philadelphia. In the year 1827, a large number of Irish emigrants were ill of a typhoid fever with ulceration of the small intestines, which was probably dothinenteritis, and during several successive years there were more or less extensive epidemics of remittent and intermittent fevers occurring in the neighbourhood of the city, but not often extending into the central parts of the town. In the winter of 1835-6, a form of fever not commonly met with at the hospitals was observed from time to time. It was characterised by pungent burning heat of the skin, dusky aspect of the countenance, subsultus, delirium, with great stupor and prostration, but there was no diarrhœa, and but few symptoms referrible to the alimentary canal. It was the disease which afterwards appeared as an epidemic. . . . The evidence of contagion was direct and conclusive; three of the principal nurses and about a dozen assistant nurses, besides a number of patients ill with various

diseases were taken with the fever. There was only one nurse of a ward in which many of the patients were collected who escaped; but several of his assistants and patients were taken ill. The wards in which the fever patients were placed were large and well ventilated. The contrast between the two fevers in this respect (their infectious character) is obvious. . . . . *Season of the year.*—The epidemic began in March and continued until August—there were a few scattering cases afterwards. The summer was unusually cool, and the spring and winter cold. . . . . *Pathological anatomy.*—In this large number of autopsies, amounting to about fifty, there was but in one case, and that doubtful in its diagnosis, the slightest deviation from the natural appearance of the glands of Peyer. . . . . The fact that the morbid changes pathognomonic of dothineritis are not met with in the typhus fever, would of itself seem conclusive that the two diseases are no more identical than pneumonia and pleurisy. Although in some respects the two affections are analogous and even similar; the radical difference of anatomical lesions is at least as well marked as the distinction between the symptoms. It is indeed singular that there should be of late a strong tendency to confound two fevers which were regarded as entirely distinct by some of the older physicians.”

In the above quotations we see strongly marked the differences of the two affections as to prevailing season—symptoms, pathology, and mode of transmission, and the similarity of each to one or other of the two forms of European fever.

Having endeavoured to collect and arrange the testimony of the best authorities as to the *sources* of the fever poison, we stop upon the threshold of the extensive inquiry into the laws which regulate the diffusion of the disease in an epidemic form.

To attempt this would require the fullest investigation into the differences and analogies of the two affections, their modes of combination in the same individual, and their occurrence in an inter-current mode during the same epidemic period, all of which modifications of disease would be found reconcileable with the theory of two poisons; the one having its elements in the blood, and reproduced in it; the other a product of putrefactive decomposition, and not reproduced in the human body; while on the other hand Dr. Davidson's recent essay contains in itself proof that his own theory of a single typhoid poison is not tenable, since it involves the assertion of the identity of two diseases, one of which (according to him) requires to be kept up by an uninterrupted series of cases of contagion, while the other, according to the best observers, never propagates itself by contagion at all. In short, according to this doctrine, we must believe that the same poison, shall at the same time and place, and among the same collection of individuals, produce two diseases totally dissimilar in their mode of access, symptoms, pathology, treatment, and mode of transmission.

## ERRATA.

- Page 95—line 9, *for account, read amount.*  
" 106—line 9, *after on the, insert contrary.*  
" 112—line 27, *for violent, read violet.*  
" 115—line 42, *for committed, read admitted.*  
" 125—line 7, *for receiving, read discerning.*  
" 130—line 4, *for Bruserin, read Burserius.*  
" 133—line 11, *for epidemics, read epidermis.*  
" 135—line 34, *for epidemics, read epidermis.*



